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THE NOMENCLATURE AND CLASSIFICATION OF SEDIMENTARY ROCKS

JOHN RODGERS

ABSTRACT. According to the purposes they serve, classifications of sedimentary rocks may be classified into descriptive and genetic, and these further into purely descriptive and analytical descriptive classifications and into purely genetic and operational genetic classifications. No one classification can serve all purposes; at the least a descriptive and a genetic classification are needed. Moreover, field workers need primarily a purely descriptive classification and laboratory workers an analytical descriptive classification. A field classification might well be built around the recommendations of the Committee on Sedimentation of the National Research Council. For a laboratory classification, the confused state of igneous rock terminology warns against the pigeon-hole type of classification. It is suggested that a continuous variable type of classification, expressed entirely by measured parameters instead of rock names, would provide precise and objective designation of rocks in terms of their properties. The paper makes no attempt to present a detailed classification but is intended to stir up discussion of the principles of classification from which, later, general agreement may emerge.

INTRODUCTION

THE subject of classification is always a live one in geology, and the classification of sedimentary rocks is being especially debated at present, as witness the symposium recently published in the Journal of Geology (Pettijohn, 1948; Shrock, 1948; Krynine, 1948). All seem agreed that the existing classification is unsatisfactory, but there agreement ceases. The wide present interest insures, however, that all attempts to create new classifications will be subjected to many-sided analysis, and we may confidently expect that from this analysis a more satisfactory classification will ultimately emerge. The present paper is intended as a contribution to that analysis of classifications; it hopes to focus attention on certain points that may deserve consideration as we work at constructing a satisfactory scheme. The paper has benefitted greatly by the helpful criticisms of Adolph Knopf, Eleanora B. Knopf, Carl

O. Dunbar, and Richard F. Flint, but I must be held accountable for all that is crude, unclear, or simply erroneous in it.

The problem of classification inevitably entails the problem of terminology or nomenclature; indeed they are but facets of a single problem. Most scientific names imply a classification, whether actually carried through or no, and many classifications consist simply in the systematic arrangement of preexisting names. I shall assume therefore that the purposes served by nomenclatures are in general the same as those served by classification, and I shall begin by discussing the several purposes of sedimentary rock classifications.

KINDS OF CLASSIFICATIONS

The first purpose to be served by a nomenclature or classification of sedimentary rocks is to provide terms that will convey from one geological mind to another an adequate conception of the physical character of a given rock. To fulfill this purpose, the terms should be descriptive, objective, and precise; genetic terms, such as till, should be avoided because they depend on hypotheses of origin and are not necessary to convey the description. A classification that serves this purpose may be called a *descriptive* classification.

A descriptive classification should provide categories for rocks of every kind, thus facilitating for teacher and student a survey of all the possible varieties that may be encountered. But in addition the classification can be of great value to the research investigator if, by grouping the like and separating the unlike, it calls attention to genetically significant correlations between likenesses and unlikenesses. The properties that might be chosen for analyzing the relationships of phenomena as complex as sedimentary rocks are of course legion, but what distinguishes a good classification of this sort from a poor one is that the properties chosen are those that best bring out such correlations and thus lead toward genetic understanding of the phenomena. In such a classification a neat balance must be struck between the descriptive and the genetic; genetic terms as such must be avoided because they beg the question and prejudge interpretation of the observed correlations, but those descriptive terms that have in fact the highest correlation with genesis will make possible the most fruitful analysis. A

descriptive classification that serves this additional purpose may be called an *analytical* descriptive classification.

A quite different purpose that may be served by a classification of sedimentary rocks is to provide terms that will convey from one mind to another an adequate conception of what is known, inferred, or believed to be the origin of a given rock. To fulfill this purpose, the terms should be genetic and unequivocal; terms that indicate the origin directly, such as till, are preferable to terms that do not, such as boulder clay. A classification that serves this purpose may be called a *genetic* classification.

A genetic classification should provide categories for rocks of every origin, thus facilitating for teacher and student a survey of all the possible hypotheses that may need to be considered. But in addition, the classification can be of great value to the research investigator if it calls attention to genetically significant criteria that may be applied in specific instances. The criteria that might serve as a basis for classifying such complex phenomena by origin are legion, but what distinguishes a good classification of this sort from a poor one is that the criteria chosen are such that the investigator can observe them unequivocally in the data available to him. To borrow a word from the jargon of the methodologists, such criteria are "operational." Here again a nice balance must be struck between the genetic and the descriptive; those genetic traits that have the highest correlation with descriptive data will provide the most useful "operational" criteria. A genetic classification that serves this additional purpose may be called an *operational* genetic classification.

Clearly the analytical and the operational classifications approach each other, and when every fact is known and every interpretation secure they will coincide to form the ideal classification toward which we are striving. But until that happy day (when all of us in sedimentary geology will be looking for other jobs), no one classification can serve all the purposes enumerated, and a classification satisfactory for one purpose, such as description, will necessarily be less than satisfactory for others, such as expressing origin. To repeat the example already mentioned, the term till gives a clear conception of the genesis of the material it refers to, but it does not provide an adequate description of that material, which might

be a strongly cohesive boulder-bearing clay, a compact but weakly cohesive silty clay with scattered chalk fragments, or a loose silty and sandy rubble. Moreover, if it is not known whether the material in question was deposited by ice or by mudflow, the genetic term cannot properly be applied and if applied will be misleading, but an objective description is still valid. On the other hand, the genetic classification conveys readily whatever can be inferred about the origin of the material and is especially helpful in suggesting additional relationships that may be looked for in checking the original inference; few if any purely descriptive classifications can accomplish this end.

For sedimentary rocks then at least two types of classification are needed, the descriptive and the genetic; neither will suffice alone. The use of descriptive terms alone would indeed make sedimentary geology a dull and mechanical subject, leading to little understanding of phenomena and hardly deserving the name of science. On the other hand, the use of genetic terms alone is not dull but dangerous; for once a term is attached to a given rock it tends to conceal ignorance and discourage re-examination, yet one of the cardinal principles of the scientific method is that no hypothesis, theory, or law shall ever be so firmly established and enshrined that it is secure from restudy, reappraisal, and perhaps rejection, whenever new facts are discovered that appear to conflict with it. If, however, the descriptive classification used is in addition analytical, then the dull process of cataloguing rock types is given point and significance and helps us to understand those types; if the genetic classification used is operational, the dangerous tendency to theorize beyond the data can be kept in check by frequent reference to objective facts. In the present imperfect state of knowledge, both classifications are needed, and neither should usurp the place of the other.

I must therefore protest vigorously against such statements as "There can be no such [descriptive] classification worthy of consideration" (Pettijohn, 1948, p. 113). Yet to quote this one statement out of context is quite unfair, for Pettijohn in the passage cited is pleading in fact for a classification in which "the characters used for the classification are meaningful or significant." To me, however, such a classi-

fication is not properly a genetic classification, as he seems to imply, but rather one of the type to which the term analytical was applied above. Pettijohn notes that the igneous petrologists do not classify their rocks by color, but he fails to note that they do classify them by such purely descriptive characters as grain size, mineral content, and feldspar composition, and in general have stopped classifying igneous rocks according to whether they are Paleozoic or Cenozoic and are tending away from the older subdivision into deepseated, hypabyssal, and volcanic. Feldspar composition seems a better basis than color because it appears to be of more genetic significance, but nevertheless the classification is strictly descriptive. That a purely genetic classification has value, I would be the last to deny, but I would suggest that the problem of genetic classification should be kept quite separate from that of descriptive classification, and furthermore that the first order of business for sedimentary geologists at the moment is to reach agreement on a satisfactory descriptive and preferably an analytical classification of our rocks.

EXISTING CLASSIFICATIONS

A touchstone for evaluating classifications of sedimentary rocks is provided by the rocks commonly grouped as limestone, for these rocks display an overall descriptive homogeneity, as shown by the use of a single field term for them, but are genetically varied. It is of some interest, therefore, to examine the disposition of these rocks in some of the principal classifications that have been proposed to replace the conventional subdivision of the sedimentary rocks.

The most uncompromisingly genetic classification ever proposed is that set forth by Grabau in his Principles of Stratigraphy (Grabau, 1913), and his treatment of limestone illustrates this very well. As Professor H. Ries once pointed out to me, the word limestone does not even appear in the index of the book, the rocks in question are scattered through four or five major subdivisions of the classification, and they bear a variety of names such as calcilutyte, hydrogranulyte, biopulveryte, etc. As a genetic classification, this is thoroughly logical, but a student wishing to learn about the rocks usually called limestone or to run down the origin of a particular one will find

it of little use. For the most part operational criteria are lacking, and the classification has been largely ignored.

In recent textbooks and articles, other classifications have been proposed as genetic by Hatch, Rastall, and Black (1938, pp. 36-39, 152), by Twenhofel (1939, pp. 260-261), and by Shrock (1948, pp. 118-119). All of these writers state that the rocks called limestone are diverse genetically, yet all of them actually group them into a single category. Surely these classifications are not genetic at all but descriptive classifications masquerading as genetic or possessing genetic subcategories, thus mixing genetic and descriptive in unsystematic fashion, and they do not differ in any essentials from the conventional classification found in most elementary textbooks of geology.

A quite different approach is that of Krynine (1948, esp. pp. 147-149), who in contrast to the men just mentioned proposes a frankly descriptive field classification. Krynine makes it evident however that he is not content with a *purely* descriptive classification but is constructing one that in the terminology suggested in this paper is analytical. This approach he applies in the first instance to the subdivision of sandstone, and one does not have to believe that there is 100 percent or even 80 percent correlation between, for instance, arkose and extreme uplift to see the great value of the method. Krynine then applies the same subdivisions to the other main groups of sedimentary rocks. Limestone he divides on two bases; first on the presence or absence of 5 percent or more of recognizably fragmental non-calcareous material and of 10 percent or more of recognizably fragmental calcareous material, second on the mineral composition of the non-calcareous fragmental fraction, using the same subdivisions as for sandstone.

This second basis seems rather less significant for limestone than for sandstone, for it depends on a minor constituent of the rock and, as pointed out to me by John A. Elson, Krynine admits it will often require studying an insoluble residue, hardly a field procedure. The first basis on the other hand might be expanded (though perhaps not in a field classification) by recognizing subdivisions based on the dominance or relative proportions in the calcareous fraction of the following four components: non-fragmental recognizably organic material,

recognizably organic fragments, inorganic fragments, apparently inorganic and non-fragmental material (which might be either the result of direct chemical precipitation or of thorough recrystallization, a distinction for which we have as yet no good operational criterion). But details are of minor importance compared with the principle to which Krynine has adhered, that of selecting as the basis of classification objective and descriptive characters, but those that have the highest correlation with genesis. Krynine's classification thus possesses a vitality that Grabau's lacked, and many of his proposals will almost certainly find their way into the classification toward which we are all working.

SUGGESTIONS FOR FUTURE CLASSIFICATIONS

Krynine's paper underlines another problem that faces the maker of classifications, the conflict between the needs of the field and the laboratory worker. The need of the field man is primarily for a purely descriptive classification, by which he can convey clearly, precisely, and objectively what he sees. Hence a field classification is more likely to use obvious characters than less obvious but genetically more significant ones. That the tendency to record obvious characters only and to ignore all the others has been overdone cannot be gainsaid, and it diminishes greatly the value of many field reports, but the field man commonly has many things to observe in a limited time beside the details of sedimentary rocks, and if the classification is too complicated or difficult he simply will not use it at all. In contrast, the laboratory worker is ordinarily primarily interested in genetic understanding, and the best *descriptive* classification for him (I leave to one side genetic classification) is an analytical one. Moreover, he is presumably equipped to determine any determinable character, and his only concern is not to waste time determining characters that have no significance. For him, therefore, it does not matter how subtle the characters used in classification are but only whether they have a high correlation with genesis; in other words, the more analytical the classification the better.

For a field classification, I would like to call attention to the classification set forth in a series of reports to the Committee on Sedimentation of the National Research Council (Went-

worth and Williams, 1932; Wentworth, 1935; Allen, 1936; Twenhofel, 1937; Tarr, 1938). Here is a classification beaten out after considerable thought and consultation among sedimentary geologists and providing for the most part simple but quantitative definitions of descriptive rock terms (a number of convenient genetic terms are also defined). The classification has no overall guiding principle except convenience and a proper respect for the coiners of terms, but it may be questioned whether phenomena as complex as sedimentary rocks will yield to any single-track classification. Probably no one will agree with all of the classification; for instance I object to the extension of the term mudstone to include not only all consolidated but all unconsolidated fragmental sediments with particles less than 1/16 mm., as I prefer the commoner restriction to a consolidated rock in that range which does *not* show fissility, but as pointed out to me by R. R. Shrock, this is not the original definition of the term, and perhaps it has been used in so many senses that it should be abandoned. Admittedly also the classification fails the limestone test miserably, for the Committee never made a proposal for the nomenclature of the carbonate rocks. But this gap could readily be filled along the lines of the existing classification; as a basis for discussion I venture to suggest a primary distinction between limestone (made of calcite) and dolomite[1]; a secondary distinction between carbonate rock made principally of organic material and that made principally of inorganic or indeterminable material; further distinction in the organic carbonate rocks according to the dominant organism, if recognizable, and according to whether the organic remains are roughly in place and whole (as in a coral reef) or mechanically transported and fragmental, if that can be determined; and further distinction in the inorganic carbonate rocks according to the presence or absence of recognizable fragmental grains or mechanical structure (such as cross lamination) and according to grain-size. In any case, the Committee recommendations could be made the basis for future discussions of a descriptive

[1] Dolostone (Shrock, 1948, p. 126) seems unnecessary, as the term dolomite was first proposed (in the form dolomie) by de Saussure fils (1792) for the rock; Dolomieu (1791) and both de Saussures (1792; 1796, pp. 17, 109, 110) write "pierre" repeatedly. As the mineralogists are the ones who have duplicated the term, they might choose some variant, such as dolomine.

field classification of sedimentary rocks, and we could attempt to sharpen up the Committee classification and fill in its gaps rather than try to start from scratch and convert the field geologist to radically new usages.

For a laboratory classification, I have no complete classification to offer, only a few negative remarks to make. Such a classification could demand relatively complete information on all the important measurable properties of sediments, and hence could use any of these properties for its subdivisions. The prevailing theory of classification seems to be to set up named compartments, quantitatively defined of course, in a grid system of 2, 3, or more dimensions, each dimension being a different parameter. If I might be facetious, I would christen a classification of this type a pigeonhole classification. Pettijohn seems to advocate (1948, p. 114) the ultimate in this type, in which each important rock type would receive its own separate name, commonly geographical; for example, bradfordite for sandstone like the one described by Krynine (1940) from well samples of an oil sand in the Bradford district, and spergenite for rock like the familiar Indiana building stone. Pettijohn points to the analogous terminology of igneous rocks, but surely the condition of igneous rock nomenclature should be a warning to the sedimentary geologists rather than a guide. The number of individual rock names for igneous rocks that had been coined before 1935 was 900 or more (Tröger, 1935, pp. 3-5), and few of these give in themselves a hint as to the kind of rock denominated. Among the igneous geologists themselves there is a growing dissatisfaction with the whole haphazard and unwieldy scheme and the burden it places on the memory (for a recent searching criticism, see Shand, 1944). In view of the unhappy experience of our colleagues, we sedimentary geologists would do well to forego the heady wine of indiscriminate name-giving (and credit-claiming) in order to avoid the hangover of a confused and illogical terminology.

As a counter to the pigeonhole type, I would like to suggest what might be called the continuous variable type of classification. An example in another field would be the modern description of the plagioclase feldspars. During the earlier period of study of the feldspars, a number of different mineral species were recognized within the plagioclase series and each was given

a name. With more study it became clear that the series is continuous from albite to anorthite, and the usage of these names was standardized to imply fixed quantitative limits, but at the same time an alternative system of designation was adopted based on percentage composition in terms of "end members." Thus a given feldspar may be described by the term oligoclase or by the symbol $Ab_{89}An_{11}$ (note that Ab and An do not stand for the *minerals* albite and anorthite but for chemical abstractions). Of these, the latter is not only shorter, but a great deal more precise. Moreover, the use of species names unduly emphasizes artificial boundaries. Why, for instance, should $Ab_{89}An_{11}$ and $Ab_{71}An_{29}$ be grouped together as oligoclase but $An_{89}An_{11}$ and $Ab_{91}An_{9}$, which are clearly more closely related, be separated as though quite different? In the case of the feldspars no one is likely to be misled, but according to several igneous rock classifications, the same distinction may cause two rocks to be separated as soda granite and granodiorite, though the entire difference between them is 2 percent in the composition of the plagioclase feldspar.

It will be objected that boundaries, even though necessarily arbitrary, are essential. But why? The rocks we are dealing with show a continuous range in most of the variables we can measure, and a classification that faces and makes use of that fact should be more useful than one that overlooks and distorts it. To be sure, we may well find breaks in such ranges that will give a basis for "natural" boundaries between classes of rocks. But in most quantitative schemes of classification, the boundaries are set up first, mainly with a view to a symmetry which the rock distribution itself does not possess, and the rocks are then expected to fit the neat pigeonholes. This is no criticism of quantitative classification as such, but only of the pigeonhole type; for example, the pigeonholes, the grads and rangs, of the C.I.P.W. system are dead and forgotten, but the continuous variable system on which they were based, the norm, is still widely used in igneous geology.

Defenders of the pigeonholes will point, however, to the convenience of group terms such as basalt, diorite, granite, for conveying the general character of the rock, which is after all the first purpose of classification as mentioned at the beginning of this paper. But that purpose is especially the province of

the field classification; the purpose of the laboratory classification is to convey not general but precise information, such as can be obtained by laboratory techniques. And again the constant confusion between field and laboratory terms for igneous rocks should give sedimentary geologists pause. It is often very difficult to tell whether, in a given paper, the term granite is being used in the precise laboratory sense of a rock consisting largely of potassian feldspar (according to some classifications it might be albite) with more than 10 percent quartz, or in the broad field sense of any light-colored coarse-grained quartzose rock. Thus we read again and again of the great granite batholiths of the Cordillera, most of which consist primarily of quartz monzonite and granodiorite. We can avoid this confusion in sedimentary geology if we will restrict the older terms such as sandstone, shale, limestone to the field classification and construct the laboratory classification on a new basis. For that basis I would like to suggest the measurement of basic parameters and their expression in formulae like those for the feldspars. Instead of first getting up our own categories and confidently pigeonholing the rocks in this one or that one, let us first study and measure the rocks and plot the measurements on triangular diagrams and other graphs, and then look diffidently to see whether there are natural groupings of those rocks that might deserve special terms.

There have been certain attempts of this sort already. I think first of Alling's paper on "microlithologies" (1945), which frankly proposes a laboratory classification of rocks without reference to the usual field terms. The classification is based on a triangular diagram, the end members of which represent the argillaceous, calcareous, and siliceous fractions of the rock, computed according to certain quantitative rules. Thus a rock called in the field a limy shale might be designated by the formula $A_{70}C_{25}S_5$. Alling has marred his own case by permitting such quantitatively meaningless expressions as "5 to 10 percent of the clastic carbonate" (p. 745) to slip in (cf. also the entirely qualitative and therefore quite unreproducible description of types of fissility, p. 753), but no one who has struggled with facies faunas can look at diagrams A and B of his figure 6 (p. 751) without realizing the power of the method.

A second proposal for the use of continuous variables in sedi-

mentary classification is that of Krumbein and his associates (Krumbein, 1948; Dapples, Krumbein, and Sloss, 1948). Krumbein also uses a triangular diagram and supplements it by ratios between the end members; these ratios permit the plotting of significant isopleth (isolith) lines on maps, as he has demonstrated. To be sure, points in his triangle refer not to individual sedimentary rocks but to summations of the rocks in stratigraphic sections of the order of magnitude of a system. The principle, however, could certainly be applied to the classification of rocks, provided the end members were quantitatively and operationally defined so that results could be reproduced from laboratory to laboratory.

Krumbein has also shown (1948, pp. 1916 ff., fig. 5) how readily the principle can be extended to all parts of a classification by subdividing each end member of the original triangle on additional triangular diagrams, and by adding additional triangles for other subdivisions, including genetic ones.

Similarly Krynine (1948, fig. 4) has illustrated the subdivision of sandstone on the basis of a triangle whose end members are mineralogically defined; thus a given sandstone might be designated as $Q_5A_{25}G_{70}$[2] (in Krynine's terms a high rank graywacke). (That his diagram shows arbitrary boundaries and that its end members are not quantitatively defined is irrelevant, for in the paper in question he was not proposing the triangle as a basis for a detailed laboratory classification but using it to illustrate the principles of his field classification.)

These examples indicate the form which a laboratory classification of the continuous variable type might take. The fundamental designation might well follow Alling's lead and be expressed in proportions of argillaceous, calcareous, siliceous, and other. Further distinctions could be based on subdivision of the dominant end members of any rock; thus the siliceous end member might be divided according to Krynine's scheme (as suggested by Krumbein, 1948, pp. 1917-1918, fig. 5), the argillaceous by the proportions of the main groups of clay minerals. Again subdivision might be made on grain size or on grain fabric, say the proportion of interlocking to non-interlocking (presumably fragmental) borders. Subdivision into three end members at each step has, of course, no special magic

[2] Standing for quartzite, arkose, and graywacke.

virtue; two or four components might be called for at various steps. (Four components can be handled on triangular diagrams by replacing the point that expresses three components by a small triangle whose size is proportional to the amount of the fourth component, which should be the least important in the given group of data, or by indicating beside each point its altitude in the four-component tetrahedron of which the given triangular diagram is the base.) The most important requirement would be that each end member be simply, quantitatively, and if possible operationally defined, so that workers in different laboratories might arrive at comparable figures. A set of three or four formulae constructed in this fashion would indicate the composition and perhaps other characters of the rock in less space and with far greater precision than the conventional terms, even if there were general agreement on quantitative limits to the pigeonholes those terms represented. For example, the formula $A_{30}C_{0}S_{70}$; $Q_{15}A_{80}G_{5}$; $K_{90}M_{0}I_{10}$[5], plus a cumulative or frequency curve of the size distribution (or a statement of the first three moments) would describe a highly feldspathic non-calcareous arkose with abundant kaolinitic cement, and at the same time would indicate precisely where in the range of arkose the given rock would fall.

CONCLUDING REMARKS

In conclusion, I venture to sum up the suggestions made in this paper. Classifications of sedimentary rocks serve a number of purposes, and no one can serve all; we need both descriptive and genetic classifications, but the need for the former is at present the greater. Among descriptive classifications we need a purely descriptive classification for field use and an analytical descriptive classification for laboratory use. The field classification might well be built around the recommendations of the National Research Council Committee on Sedimentation. For the laboratory classification, I suggest abandoning the traditional pigeonhole type of classification and attempting to replace it with formulae of the continuous variable type which, although abstract, express the data precisely and without subjective coloration.

[5] Standing for kaolin group, montmorillonite group, and "illite" group of clay minerals.

Finally, I realize that many of the proposals here made are shots in the dark and require careful study and much modification before they can hope to be considered for acceptance. If this contribution has any value, it is likely to be negative, by leading to a review of the tacit assumptions and unquestioned procedures that have underlain our attempts at classification, rather than positive, by presenting fully blown the classification to end all classifications. I believe that no one of us alone can presume to create such an ideal classification for no one of us sees all the needs, those of the field man and those of the laboratory worker, those of the teacher and student and those of the research investigator. But by working together, by friendly interchange of ideas and constructive criticism, we should be able to devise a classification or rather classifications which if not satisfactory to all in all respects will at least provide us with an adequate common language.

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THE JADEITE PROBLEM

HATTEN S. YODER, JR.

PART II

PREVIOUS EXPERIMENTAL WORK

One of the early attempts to crystallize jadeite (Doelter, 1884, p. 63) yielded only a glass. In 1887 Lemberg subjected glasses, produced by melting natural jadeite, to solutions containing NaCl or Na_2CO_3 at several hundred degrees for long periods. In all cases analcite was produced. In 1903 Doelter gave the "melting point" of jadeite as 1000 to 1060°C. Schumoff-Deleano (1913) attempted to synthesize the crystals of the diopside-jadeite series using mixtures of synthetic diopside and a natural jadeite from "Tibet." She concluded that diopside suffered no change in optical properties when crystallized from a melt containing up to 5 per cent of the jadeite component. With a mixture containing greater than 5 per cent and less than 15 per cent, diopside crystals were obtained in a groundmass of diopside, jadeite, and glass. The jadeite in the groundmass was the remnants of the unmelted natural material. A mixture containing more than 15 per cent of jadeite yielded all glass. It was stated that jadeite had not been synthesized directly nor by using sodium tungstate, sodium molybdate, or other fluxes.

Scott (1916) in his study of analcite was unsuccessful in his attempt to verify Doelter's claim of preparing "anhydrous soda-leucite" by dry fusion. He found that a glass of the jadeite composition yielded nepheline and glass, or carnegieite and glass depending on the temperature of crystallization. He doubted if an anhydrous feldspathoid of the composition $NaAlSi_2O_6$ existed.

It should be mentioned here that the "decomposition" of leucite into nepheline and orthoclase (pseudoleucite) is *not* analogous to the assumed reaction 2Jd = Ne + Ab. Bowen and Ellestad (1937) have shown that pseudoleucite is produced as a result of the following reaction: leucite + liquid = nepheline + orthoclase.

In 1922 Eskola and Adams (unpublished), working at the Geophysical Laboratory, attempted to produce jadeite from a

synthetic glass under high hydrostatic pressures (less than 4000 atmospheres), but obtained only a feathery intergrowth which Merwin said might be or might not be jadeite. In experiments using the Morey-type bombs, Eskola obtained unidentifiable fibrous products. He (personal communication to Dr. Adams, 1948) is of the opinion that the formation of jadeite should be promoted by pressure, but perhaps mineralizers are also necessary to help crystallization.

Bowen (1922) published the pseudobinary diagram of equilibria for mixtures of nepheline and diopside (fig. 6). From

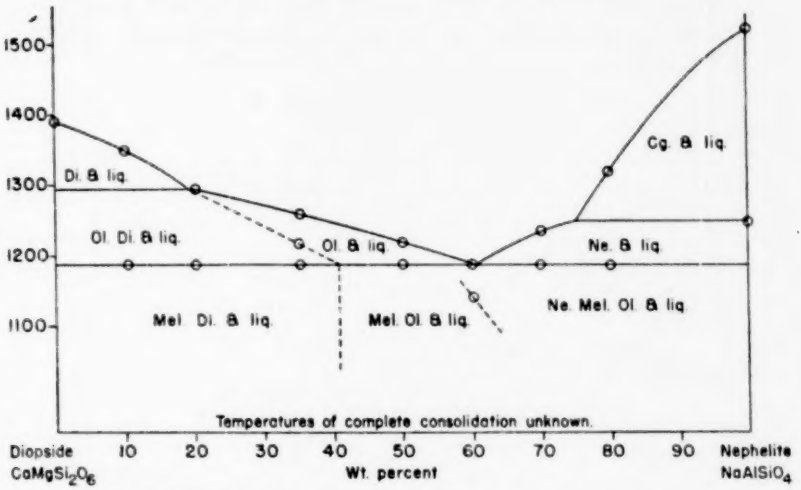

Fig. 6. Pseudobinary diagram of equilibria for mixtures of nepheline and diopside (Bowen, 1922).

these data and field observations in Quebec, he concluded that the residual nephelite-rich liquid could give rise to products of the analcite composition, i.e., jadeite.

Using the sealed capsule technique, Goranson (unpublished) in 1931 also tried to synthesize jadeite from a glass in the presence of water vapor. He recorded the following notes:

	Original Material	Per cent H_2O in capsule	Products
1.	Synthetic glass	4.26	glass + rod-like fibers and possibly plates. Negative elongation and extinction parallel to elongation.

2.	Synthetic glass	13.47	glass + numerous hexagonal plates of nepheline + rods of albite (?). Some of nepheline crystals appear altered.
3.	Burma jadeite	12.05	glass + crystalline material. Rods like in No. 1. Negative elongation.
4.	Burma jadeite	3.10	glass + crystalline material. Rods and possibly another phase.

These were held for three hours at 800 ± 10°C. under 2000 kg/cm² pressure. No jadeite was produced.

Greig and Barth (1938) determined the binary diagram, nepheline—albite (fig. 7), in which the composition of jadeite

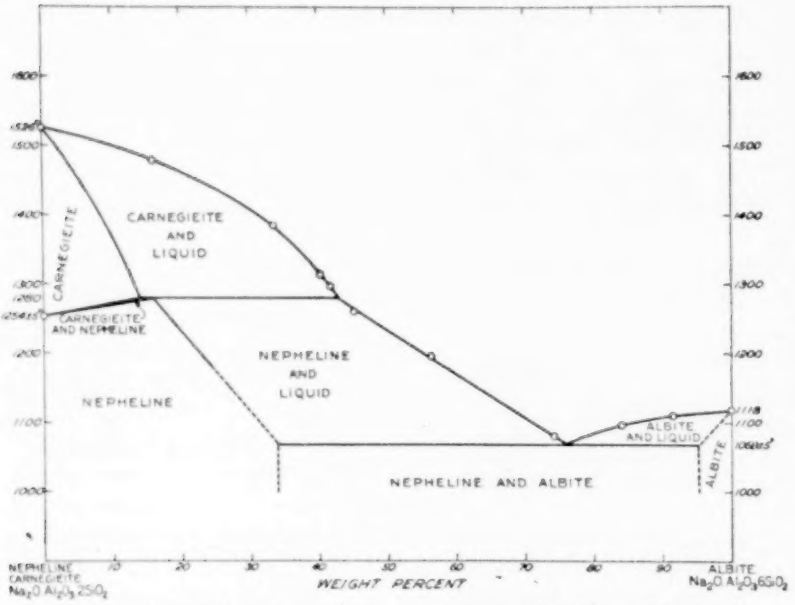

Fig. 7. Equilibrium diagram for the system, $NaAlSiO_4$ — $NaAlSi_3O_8$ (Greig and Barth).

occurs (Ne 35.14 per cent). No crystals of jadeite were found in any of the preparations. In an effort to decide whether jadeite was stable or not under the conditions of the experiment, they heated a natural jadeite from Burma (USNM #94303) and found that it decomposed as low as 800°C. After having been heated at 1015°C., the natural material was completely

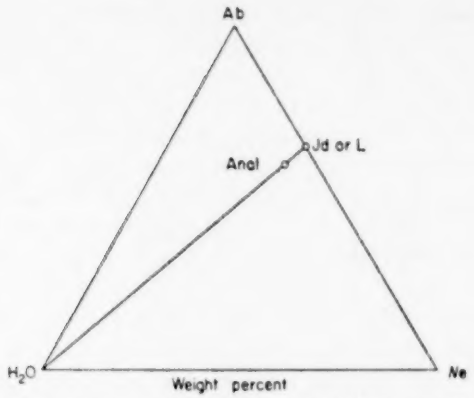

Fig. 8. The system, H_2O — Nepheline — Albite.

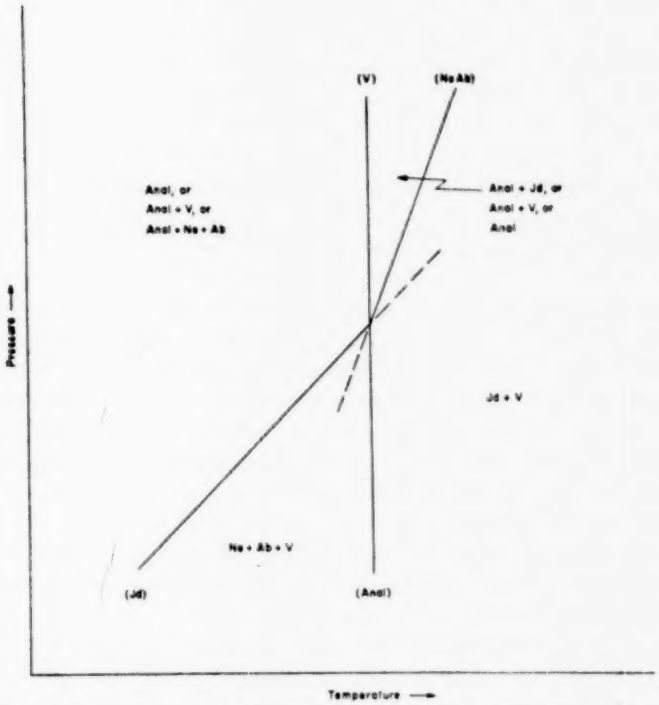

Fig. 9. Case I. Theoretical relations in the system, H_2O — Nepheline — Albite (after Morey).

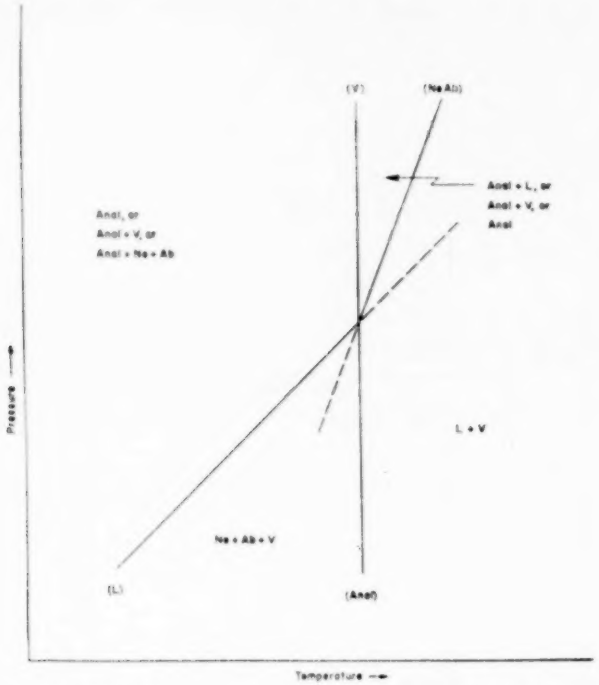

Fig. 10. Case II. Theoretical relations in the system, H_2O — Nepheline — Albite (after Morey).

changed to glass plus large nepheline crystals. They concluded that jadeite was not stable under the conditions of the experiment. The solidus temperature of the jadeite composition was fixed at 1068 ± 5°C. and the liquidus temperature at 1138°C.

Foster (1942) also failed to obtain jadeite in the system albite—wollastonite—nepheline. He said this was "compatible with the general belief that jadeite requires considerable pressure for its formation."

Morey (unpublished, 1947) subjected natural jadeite, analcite, and synthetic glass of the jadeite composition to 400°C. and approximately 1000 atmospheres water vapor for several days in the Morey-type bombs. The natural material was unchanged, the analcite was unchanged, and the glass had completely crystallized into analcite. From these and other experiments, Morey concluded that stable equilibrium had not

been obtained and therefore the results were inconclusive.

In addition to this experimental work, Morey made a detailed theoretical study of the system H_2O—nepheline—albite (fig. 8). He assumed that three components were sufficient to express the composition of all phases; that is, that all phases were in the plane H_2O—nepheline—albite, or H_2O—$Na_2O \cdot Al_2O_3$—SiO_2. The coexistence of four phases, therefore, would give a monovariant system represented by a curve on the P-T diagram. This curve would be terminated, according to Morey, at an invariant point at which another phase is formed. This phase may be either a solid or a liquid such as would be formed at a congruent or incongruent melting point, but it was assumed to be in the plane H_2O—nepheline—albite. The first case is given in figure 9 where the solid phase is assumed to be jadeite. Only compositions along the H_2O—jadeite join are considered here. The curves are designated by the phase absent; the fields by the possible combinations of phase present. From a qualitative approximation of the entropy and volume differences, it was shown by Morey that in this case a field of jadeite exists at atmospheric pressure; therefore jadeite cannot be considered as a pressure mineral. If a liquid, such as at a congruent melting point, is formed at the quintuple point, the picture becomes that of figure 10. Again only compositions along the H_2O—liquid join are considered, and the composition of the liquid is assumed to be that of jadeite. It should be noted that these are not the only possible relations in the system H_2O—nepheline—albite.

Bowen and Tuttle (unpublished, 1947), using the Tuttle quenching apparatus, subjected a synthetic glass of the jadeite composition to the following conditions.

58F	710°C.	15,000 psi	1 hr.	nepheline (?), albite, and glass. rare crystals.
59A	710	30,000	1	nepheline (less than 5 per cent) + albite (rare) + glass.
59C	610	30,000	2.5	nepheline + albite + glass. crystals less than 5 per cent.
59D	500	30,000	5	analcite 100 per cent.
59G	550	30,000	1	analcite 100 per cent.

While these experiments were in progress Schairer (unpublished, 1947) determined the liquidus conditions for the plane

diopside—nepheline—silica (fig. 11) and some of the relations in the pseudobinary system jadeite—diopside (fig. 12). In the latter system, the phases present below the liquidus were determined in part by x-ray diffraction powder patterns. The diopside—nepheline—silica diagram bears out the conclusions of Bowen (1922). It is significant that a mixture in the forsterite field can yield a residual liquid of approximately the jadeite composition. In the two known field occurrences of jadeite, the

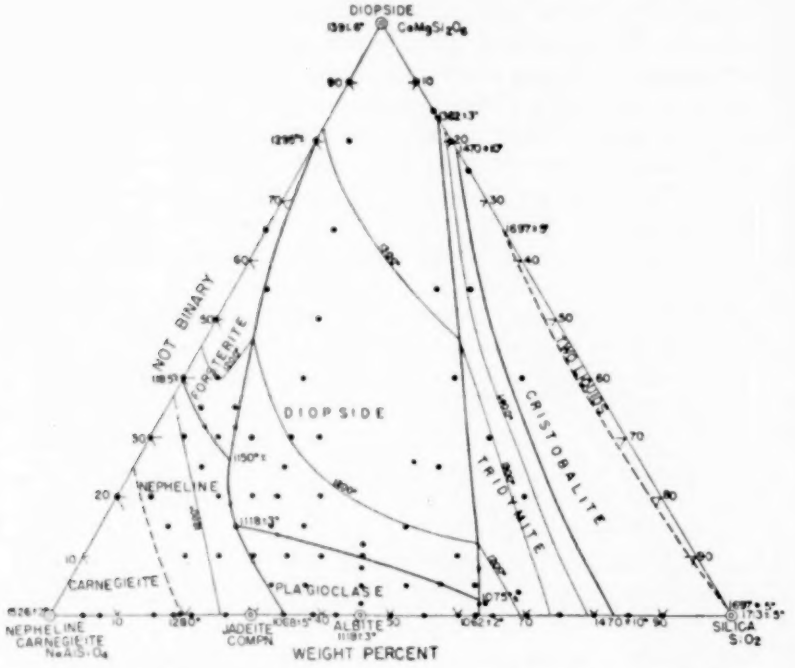

Fig. 11. Preliminary diagram of the system, $CaMgSi_2O_6$—$NaAlSiO_4$—SiO_2 (Schairer, unpublished).

pyroxene occurs in a serpentine. There is the suggestion, then, that the jadeite formed as a residual product of the process which gave rise to the serpentine. Bowen and Tuttle (1949) have fixed the upper limit of the field of stability of serpentine in the vicinity of 500°C. Jadeite, if this reasoning is correct, would have formed at a temperature less than 500°C.

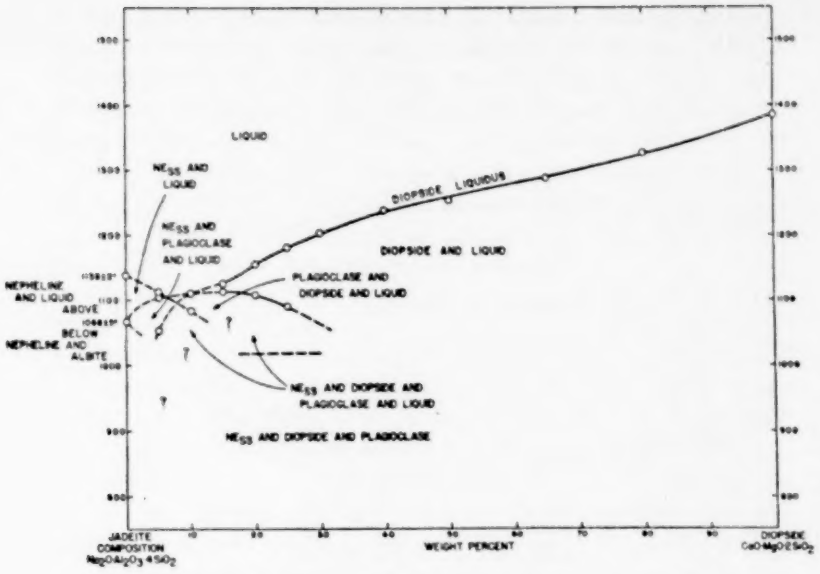

Fig. 12. Preliminary diagram of the join, Jadeite — Diopside (Schairer and Yoder, unpublished).

In addition, Schairer and Merwin (unpublished, 1948) investigated the decomposition of two natural jadeites from Burma (USNM #94829 and #94303). On the basis of heatings two weeks long they found that both specimens showed signs of decomposition into material of a lower index (less than 1.53) at a temperature as low as 850°C. The observation was made that the natural jadeite first melts completely metastably and then crystallizes into nepheline and albite. The liquidus temperature of the natural material (USNM #94829) was determined to be 1128°C. below which nepheline first forms.

Schairer and Yoder (unpublished, 1949) have also made exploratory runs on the jadeite—acmite join (fig. 13). All mixes at 800°C. which had completely crystallized contained acmite (or acmitic pyroxene), nepheline solid solutions, and albite. Present data indicate that there is a lowering of the incongruent melting point of acmite (990°C.). The phases present below the liquidus were determined in part by x-ray diffraction powder patterns.

Recent experiments of Bowen and Tuttle (unpublished, 1949) indicate that there are two forms of albite. Although they have not been able to synthesize the low form, there is evidence that the transformation takes place as low as 700°C.

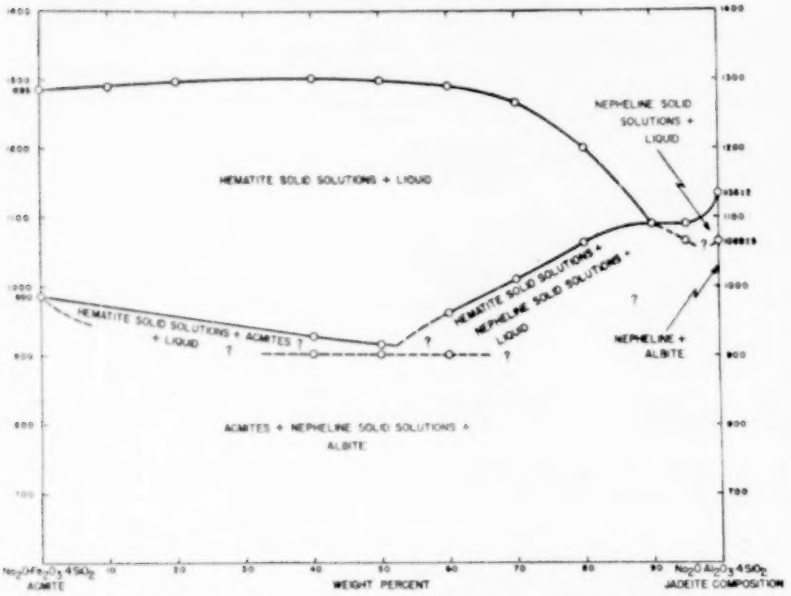
NEPHELINE SOLID SOLUTIONS + LIQUID
HEMATITE SOLID SOLUTIONS + LIQUID
HEMATITE SOLID SOLUTIONS + LIQUID
HEMATITE SOLID SOLUTIONS + NEPHELINE SOLID SOLUTIONS + LIQUID
NEPHELINE + ALBITE
ACMITE + NEPHELINE SOLID SOLUTIONS + ALBITE
WEIGHT PERCENT
ACMITE
JADEITE COMPOSITION

Fig. 13. Preliminary diagram of the join, Acmite — Jadeite (Schairer and Yoder, unpublished).

Tuttle examined a specimen of albite which contained some jadeite from Tasco, Querrero, Mexico, loaned to the writer by W. F. Foshag. The albite was of the low form such as is found at Amelia County, Virginia. This suggests that the jadeite may have formed at temperatures at least as low as 700°C.

FURTHER EXPERIMENTAL WORK

Thermal. Synthetic glass of the composition of jadeite ($n = 1.498$) was prepared for the writer by Dr. J. F. Schairer. The index agreed exactly with that obtained by Kani and Hosokawa (1936). Care was taken to anneal the glass at successively lower temperatures (to 1150°C.) with intermediate crushings. Such procedure was found helpful in increasing the rate

of crystallization of albite from its glass. The glass was then held for two months at 500, 600, 700, 800, 900, and 1000°C. At 800°C. and below the glass did not crystallize. At 900 and 1000°C. the glass crystallized into nepheline and albite.

Natural jadeite held at the same series of temperatures was unchanged at 800°C. and below. At 900 and 1000°C. it completely decomposed into nepheline and albite. This agrees with the shorter runs of Schairer on the same specimen.

Powders of the jadeite composition consisting of sodium carbonate, alumina, and quartz were pressed under five tons per square inch and sintered at the same series of temperatures. No change was observed at and below 800°C.; at 900 and 1000°C. small clusters of nepheline and albite were found to have grown about the grains of alumina.

Fluxes. Synthetic glass was heated at temperatures above the melting points of sodium metavanadate (630°), sodium tungstate (698°), and sodium molybdate (687°). In all cases nepheline and albite crystallized.

Volatiles. Small amounts of synthetic glass were placed in sealed silica tubes with a few grains of a material which when heated at a requisite temperature would volatilize or release a volatile component. The following substances were tried:

NaF	500°C.	216 hrs.	fluoro-sodalite + albite
$Na_2Cr_2O_7$	500°	29	chrome-noselite + albite
Perlite (H_2O)	500°	29	no change
$Al_2O_3.xH_2O$	500°	166	no change
$Al_2O_3.xH_2O$	700°	75	nepheline + albite
I_2	500°	61	no change.

The pressure existing in the tubes was probably not more than a few atmospheres.

Synthetic glass was subjected to various hydrothermal conditions in the Tuttle quenching apparatus; the data obtained are presented in table 2 and plotted in figure 14. The solid line is the equilibrium curve between analcite and nepheline + albite + H_2O. The albite obtained in these runs gives a powder pattern similar to that of synthetic albite obtained in other systems. It differs, however, from the pattern obtained from some natural albites. The analcite produced gave a powder pattern similar to that found in the Hanawalt File (II-932) within

TABLE 2

Hydrothermal Treatment of Synthetic Glass of Jadeite Composition

Temperature °C.	Pressure p.s.i.	Time hrs.	Products (per cent present)
500	7500	1.5	Anal. (90)
525	7500	96	Anal. (50), Ne + Ab (50)
550	7500	96	Ne + Ab (80)
380	15000	40	Anal. (100)
510	15000	24	Anal. (100)
525	15000	24	Anal. (100)
550	15000	24	Ne + An (1)
{ 515	15000	16	Then T raised to 560
} 560	15000	76	Anal. (75), Ne + Ab (25)
575	15000	1.2	Ne + Ab (10)
590	15000	18	Ne + Ab (90)
800	15000	69	Ne + Ab (50)
885	15000	6	Ne + Ab (rare)
900	15000	1	Ne + Ab (90)
950	15000	1	Ne + Ab (1)
985	15000	1	Ne + Ab (rare)
1025	15000	2.5	Glass
1040	15000	1	Glass
550	22500	24	Anal. (100)
550	30000	24	Anal. (100)
575	30000	24	Ne + Ab (1)
575	38000	0.5	Anal. (100)

the error of measurement. The dashed curve represents exploratory runs made to determine the equilibrium between nepheline + albite and liquid. No jadeite was encountered in any of the runs. It should be apparent that in the quenching apparatus only the fields nepheline—analcite—H_2O and albite—analcite—H_2O can be realized below the solid curve. It is conceivable that jadeite could form in the analcite range in an environment deficient in water with respect to analcite. The sealed capsule technique of Goranson could be used to investigate this region, however another method was tried but not explored fully. The method consisted of pressing a mixture of analcite and jadeite glass into a small, thick-walled, metal container under many tons pressure. This packing procedure was repeated until the container was completely filled. The container was then sealed with a silver washer and heated at 500°C. It was reasoned that the mixture would be deficient in water with respect to analcite, and therefore represent a point on the analcite—jadeite join. No change was observed in the mixture after five days' heating; no jadeite

was produced. Perhaps the use of other materials, smaller grain size, and longer heatings at various temperatures would prove fruitful.

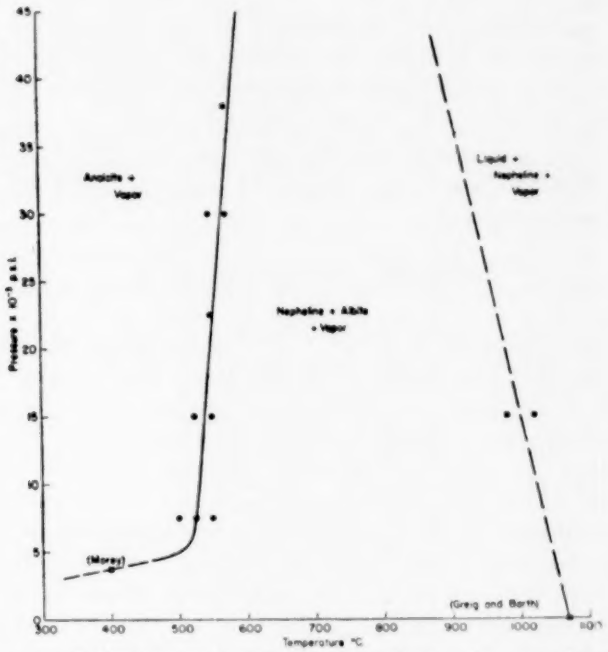

Fig. 14. Hydrothermal treatment of synthetic glass of the jadeite composition.

Several runs were also made with powders of the jadeite composition consisting of such materials as sodium disilicate, $Al_2O_3.xH_2O$ (approximately 8 per cent H_2O by weight), quartz, and crystallized sodium tetrasilicate glass. The products were the same as those obtained with synthetic glass.

Natural crystals of jadeite from Burma (USNM #94829) were also subjected to various hydrothermal conditions in the Tuttle apparatus. In general the results were the same as those for the synthetic glass. Below the solid curve the jadeite was completely altered to analcite; above the curve it was decomposed completely into nepheline and albite.

Thermodynamics. In anticipation of a direct experimental determination of the pressure and temperature conditions under

which jadeite is stable, an indirect thermodynamic approach can be made. The approach is essentially that of determining the thermodynamic properties of the phases in the assumed reaction: nepheline + albite = 2 jadeite. The P and T conditions under which this reaction may go can be computed from these properties. Whether the reaction will actually go or not, that is, if the rate is sufficient, cannot be determined at present.

It is of interest to outline the information necessary to make such a computation. The criterion that a reaction may go is that the free energy change be negative ($\Delta F < 0$). For the above reaction:

$$\Delta F = 2F_{Jd} - (F_{Ne} + F_{Ab}) < 0.$$

It is not convenient to compute the free energy of each phase directly, therefore the following relation is used:

$$d(\Delta F) = \frac{\partial(\Delta F)}{\partial T}\bigg|_P dT + \frac{\partial(\Delta F)}{\partial P}\bigg|_T dP \quad .$$

The coefficient of dT in the first term of the expansion is exactly equal to $-\Delta S$ (S = entropy) which may be evaluated from heat capacity data. The coefficient of dP in the second term of the expression is exactly equal to ΔV (V = volume). The volume at a specific temperature and pressure may be obtained by determining the terms of the expansion:

$$dV = \frac{\partial V}{\partial T}\bigg|_P dT + \frac{\partial V}{\partial P}\bigg|_T dP + \frac{\partial^2 V}{\partial P^2}\bigg|_T dP^2 + \frac{\partial^2 V}{\partial T^2}\bigg|_P dT^2 + \frac{\partial^2 V}{\partial P \partial T}\bigg|_{T,P} dP dT$$

The first term is the thermal volume expansion and the second term is the compressibility. Since these two terms are essentially linear, the third and fourth terms are negligible. The last term is also considered to be negligible, but its value may be estimated with some accuracy if necessary.

In summary, it is necessary to have the following data for each phase of the assumed reaction before its free energy may be computed; entropy at 298°K., heat capacity constants, heat of formation at 298°K., compressibility, and thermal volume

expansion. Various laboratories are now determining these values.[1]

DISCUSSION

As stated at the outset, jadeite has been regarded as a "high-pressure mineral" mainly on the argument that it occupys a smaller volume than its chemical equivalents, nepheline + albite. In the preceding section it was recalled that the free energy of a reaction is a function of both volume *and* entropy. Therefore it is not sufficient to consider only the volumes of the phases. Furthermore the volume is a function of both temperature and pressure. In fact, the usually disregarded thermal expansion term is of greater magnitude than the compressibility term; the differentials in a reaction of several phases would be accordingly affected. It is, therefore, the opinion of the writer that to assume pressure as the sole agent which drives a hypothetical reaction is an unwarranted inference. If a reaction is known to be possible on other grounds, it will proceed under pressure in the direction of smaller volumes. However, the mere fact that the supposed products are of smaller volume than the reactants is not sufficient basis for inferring that the reaction will take place. These arguments do not preclude the possibility of jadeite being a high-pressure mineral.

The field occurrence of jadeite, as lenses and masses in a contact zone, suggest that it formed under special local conditions. The fact that it occurs in veinlets penetrating the wall rock might indicate conditions which are usually referred to as pegmatitic. One interpretation of the fragment of nepheline containing needles of jadeite is that the jadeite formed first under a set of conditions which changed to a range in which

[1] Dr. K. K. Kelley (personal communication, 1949) has obtained provisional values for the change of standard entropy at 25°C. for the following reactions:

(1) $2Jd \rightarrow Ab + Ne$

(2) $Jd + Qtz \rightarrow Ab$.

If the heat of reaction in (1) is greater than $\Delta H_{298.16} = 4380$ and in (2) is greater than $\Delta H_{298.16} = 2210$, then jadeite would be stable at room temperature. The magnitudes of these heats of reaction are not unreasonable, and, Kelley says, the "signs of the required heats of reaction are those that would be predicted in consideration of the high density of jadeite." The heats of formation are now being obtained by Kracek and Neuvonen at the Geophysical Laboratory.

nepheline and albite were stable before crystallization was complete. The analcite dikes containing crystals of sodic pyroxene have already been mentioned. Another interpretation is that in the assumed reaction $2Jd = Ne + Ab$, the combinations $Jd + Ne$ or $Jd + Ab$ are possible equilibrium associations. The local predominance of associated minerals and mineral combinations might be claimed as either due to inhomogeneity or to non-equilibrium. The Burmese and Japanese occurrences of jadeite might be analogous to the occurrence of anthophyllite as a contact mineral around serpentine bodies. Anthophyllite has been shown by Bowen and Tuttle (1949) to be metastable under laboratory conditions but may have been rendered stable in nature by the addition of other constituents. In this connection, the common occurrence of acmitic pyroxenes has already been mentioned.

The structural analysis leads to the conclusion that jadeite is a pyroxene with the type structure of diopside. It also suggested that the field of stability of jadeite probably lies in a region of low temperature. Further, it may be dependent on high pressure or may be hydrous. The stability of the jadeite structure is not believed to be dependent on "structural wedging" by some "impurity" but may owe its existence to the presence of iron, hydroxyl, or an excess of aluminum.

The inability to synthesize jadeite at atmospheric pressure has been an argument in favor of the necessity of pressure. However, not all the standard methods of crystal synthesis have been exhausted nor is the field closed to new technique. Jadeite is, of course, not the only pyroxene which has not been synthesized. The lithium analogue of jadeite, α-spodumene (monoclinic), has not been produced with certainty in the system eucryptite—silica, although a substance having similar properties has been synthesized (Roy, Roy, and Osborn, 1949). On the other hand β-spodumene (hexagonal) is readily obtained. Oddly enough, α-spodumene occupies 23 per cent less volume than its chemical equivalents eucryptite + quartz. It can be said that jadeite has not been produced from synthetic or natural materials in the presence of catalysts, under moderate pressures, or in related systems.

The experiments completed do limit the possible range of stability of jadeite. A consideration of the metastable melting

of natural jadeite gives rise to the following picture. Assume that the material X lowered the melting point of the nepheline-albite mixtures such that a field of jadeite appeared (fig. 15).

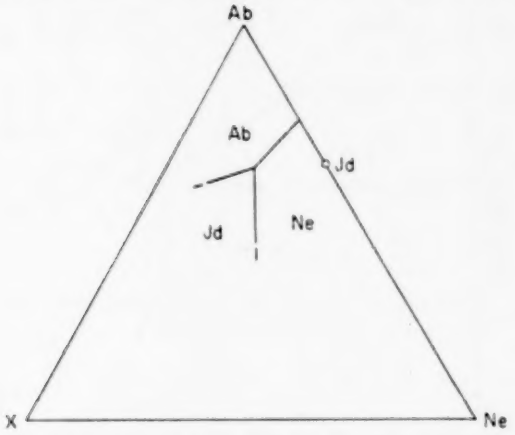

Fig. 15. Hypothetical field of jadeite in the system, X — Nepheline — Albite (after Bowen).

The projection of this surface (fig. 16), representing the liquidus of the jadeite field, would intersect the nepheline-albite binary at the metastable point of jadeite. One possible inter-

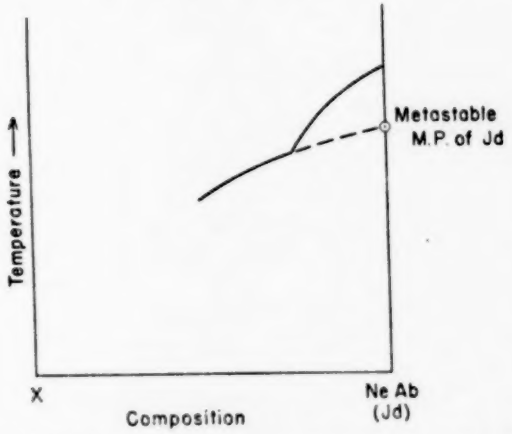

Fig. 16. Hypothetical pseudobinary, X — Jadeite (after Bowen).

pretation would be as in figure 17. Considered as a function of

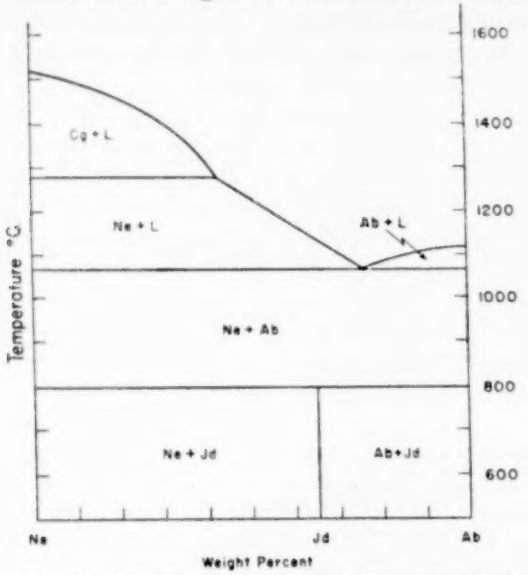

Fig. 17. Hypothetical binary system, $NaAlSiO_4$ — $NaAlSi_2O_6$ (modified and simplified from Greig and Barth, 1938).

pressure the picture becomes that of figure 18.

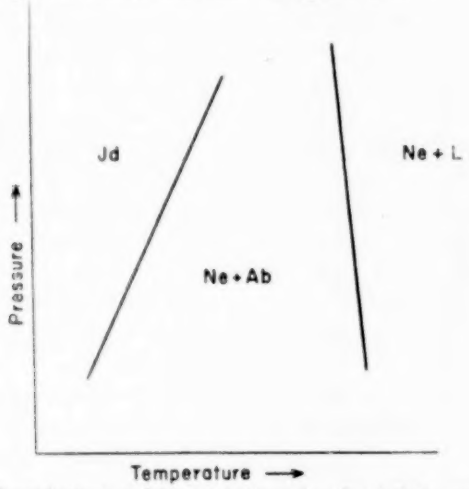

Fig. 18. Hypothetical stability relations for the jadeite composition as based on figure 17.

Using the principles of Schreinemakers (1916) and others, the metastable melting may be explained in another fashion. Consider the P-T relations of the four phases Ne, Ab, Jd, and Liquid. The sequence of curves about an invariant point can be shown to be as in figure 19. A two-component system was

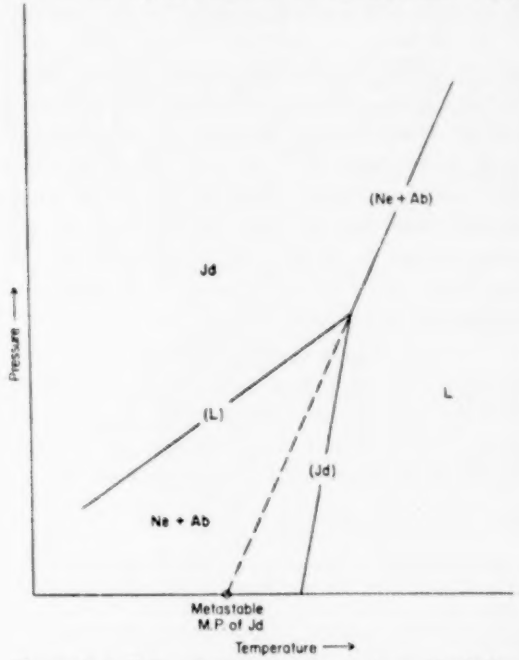

Fig. 19. Theoretical P-T relations in the system, Nepheline — Albite.

assumed; only the associations of the jadeite composition are represented, and the composition of the liquid was taken to be the same as jadeite. If curve (L) intersects the ordinate axis, jadeite is indeed a pressure mineral. The metastable reaction Jd = L as drawn might possibly be realized at atmospheric pressure.

When all possible compositions are considered, it is seen that the association Jd + Ne + Ab is theoretically impossible in any system except at the decomposition temperature of jadeite as represented in figure 17. The fact that Bauer (1896) found such an association could have several explanations. It could be argued that the presence of another component,

for example CaO in the feldspar, would permit a three-phase equilibrium. It is possible that the jadeite was formed exactly at the triple point. Still another reason might be that equilibrium was not obtained; that the reaction did not go to completion. On the other hand, it might suggest that the reaction Ne + Ab = Jd is not a valid assumption for the formation of jadeite.

Perhaps the most fruitful approach to the jadeite problem is in the system Anal—Ne—Ab. Considerable use could be made of apparatus which would permit the regulation of water present at a wide range of P and T. There is the possibility that jadeite requires either H_2O or OH in its structure, but there is little doubt that an "anhydrous" jadeite exists.

In conclusion, the writer believes that the field of stability of jadeite probably lies below 800°C. and that volatiles are necessary to promote its formation.

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SPECIES CRITERIA IN OSTEOSTRACI, WITH SPECIAL REFERENCE TO THE GENUS TREMATASPIS

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ABSTRACT. An attempt is made to compare species criteria as used in the Genus *Trematapsis* with those used in establishing species in the other genera of the order *Osteostraci*. Exception is taken to Denison's use of histologic characters of the shield as an adequate basis for placing specimens which differ in other respects in the same species.

ONE of the problems which arises to plague every practicing taxonomist is that of establishing valid criteria for the delimiting of species. Few of us would admit serious doubt as to the validity of the species concept, but certainly the criteria we use in our attempt to establish the identity of the species are rather arbitrary. There is an old statement somewhere to the effect that "God made the species, man the higher categories," but if that were true it would have helped mightily if the species had been more carefully labelled. The criteria we use in setting up our taxonomic categories may not be "man-made" but they are definitely "man-selected."

One of the most realistic definitions of species which I happen to have seen is that of Tate Regan (1925), which can be paraphrased to state that a species is any group which has been so designated by a "competent taxonomist." Huxley (1942), in commenting on that definition points out that the definition of competent needs some clarification. Perhaps it would be better stated that a species is any group which has been so designated by a competent taxonomist who has worked with the group under discussion.

We find, to be sure, that there are disputes among the "competent taxonomists" working in the same group as to what shall be valid criteria, but it would seem as though such questions could be settled by following principles which the workers on that group could agree upon. Whether it will ever be possible to establish certain "grades of organization" which can be applied to species criteria throughout the animal kingdom, or even through entire classes or phyla, is questionable, although closer agreement than present schemes indicate would certainly be desirable. Probably no biologist would deny that

the number of species of insects is justifiably enormous, but there would be found, I believe, a considerable number of us who would question whether the concept has exactly the same significance that it has in mammalogy.

Even within the class Mammalia the number of species recognized increases markedly as one goes from the large to the small forms. There are many more species of rodents recognized than there are of carnivores. It may be another valid question whether a "species" among rodents is the morphological equivalent of a "species" among carnivores, *i.e.* whether they recognize similar "grades of organization."

When, as in the present paper, we deal with fossil forms there are still other problems which bedevil the would-be systematizer of the systematists. On the strictly morphological level to which we are almost invariably limited in dealing with fossil forms it has been pointed out that the type of criteria used in specific distinctions when dealing with living forms are frequently unavailable when we deal with fossils. In discussing with another taxonomist the problem of separating species in the *Trematáspidae*, during the period when I had my 1938 paper on that group under preparation, he remarked that he believed one was justified, in dealing with fossils, in recognizing as distinct species any form which could be consistently recognized by the investigator. In his account of the history of land mammals, Scott (1937) wrote that he believed that what we recognize as species in fossil mammals would be more nearly equivalent to the genera of the neo-taxonomist. I would not advocate adopting that procedure. It would result in an enormous amount of confusion to do so.

Another difficulty faces the paleo-taxonomist when he attempts to correlate his work with that of the neo-taxonomist. Population genetics seems to be entering the thinking of the taxonomists, and rightly. Simpson (1943) has stressed the concept that among modern forms a species is a breeding population. How far can we go in the recognition of such populations among fossils? How great a stratigraphic range should be regarded as justifiably included in the same breeding group?

In his scholarly discussion of "Tempo and Mode in Evolution" Simpson (1944) has made an interesting attempt to bring genetics and paleontology somewhat closer together. It has

occasionally been possible to recognize in fossil series what are apparently the effects of single mutations. Simpson has discussed the case of digit reduction in the horse series from that view-point and seems to have made out a good case for it. But even in neo-biology we are still far from reaching any agreement as to what degree of genetic change constitutes specific distinction. It would be very convenient if we could erect a scheme based on mutations, such that so many mutations constituted sub-specific distinction, a certain additional number differentiating species, etc.

Growth stages bring in still another problem for the paleontologist. Bashford Dean (1899) once discussed the suggestion that *Paleospondylus* might be a larval "lamprey." Among some incomplete notes left by Dr. Wm. Patten were some sketches in which he appeared to be trying to correlate the structure of the same creature with known larval forms, probably Tunicate. In 1947 Denison suggested that one species of *Trematapsis* which I had separated, *T. patteni* (Robertson, 1938), was based on what probably were juvenile specimens of *T. mammillata*. Our inability to follow through the development of these creatures leaves uncertainty as to how far we can go in identifying growth stages.

In 1945 I published a discussion of cephalaspid genera and species. The present paper is an extension of this discussion to include other *Osteostraci*. It is an attempt to see whether it is possible to set up a standard set of criteria for family, genus, and species within the entire order. It has seemed to me that the *Osteostraci* would lend themselves well to such an attempt, in that there is a rather close adherence to pattern within the order, with an apparent similarity of habitat throughout.

The more immediate stimulus to make the attempt comes from the above-mentioned paper by Denison (1947), in which he suggested certain revisions of the species of *Trematapsis*. It has seemed to me that it should devolve upon me either to acknowledge his strictures on these species as justifiable or to show to what degree I regard his criticisms as unsound. The question in this case should be settled only after analysis of the broader question of species criteria in the *Osteostraci*.

In my discussion of cephalaspid genera and species (1945) I

laid emphasis on one aim of taxonomy. When we classify any group of objects we are attempting to bring order into the grouping so that when any of the terms of reference are used they will have definiteness of meaning. In other words, one of the major aims of taxonomy is specificity of reference. It is sometimes necessary to revise a classification, but it does not appear to be justifiable to change it because of slight differences in viewpoint, or on the basis of some theoretical construction which represents simply another hypothesis. In general, according to my philosophy of taxonomy, uniformity among workers with a particular group is at least a major consideration and should not be sacrificed without a very thorough consideration of the viewpoints of all workers concerned.

My present view regarding the origin of species is simply that, generally under conditions of isolation, geographic, ecologic, etc., mutations have accumulated in groups previously conspecific to such an extent that students of the group would recognize them as specifically different. In some cases environmental selection has played a definite role in the separation, especially when one group, either the parent or an off-shoot, has become extinct, and in every case selection plays a part in the ensemble of characters which constitute the distinct species, since the organisms constituting the species had to be "going concerns" in their environment, but even when selection did not influence the distinction between the species isolation would tend to result in differentiation.

There are recognized at present the following families of *Osteostraci*: *Cephalaspidae*, *Trematapidae*, *Dartmuthiidae*, and *Oeselaspidae*. In my 1945 paper, I suggested that *Didymaspis* should perhaps be raised to family rank, adding thus a fifth family, the *Didymaspidae*. *Didymaspis* has been variously placed. Recent workers have generally listed it under the *Cephalaspidae*. Romer listed it under the *Dartmuthiidae* (1945, p. 572).

Comparison of the papers of recent workers in the taxonomy of the cephalaspids indicates that these workers in general have used the following list of criteria of species:

(1) Shape and proportions of the shield;

(2) Configuration of the rostral margin of the shield;

(3) Shape, direction, and ornamentation of the cornua of the shield;
(4) Shape and size of the "pectoral sinuses";
(5) Shape and extent of the "interzonal" region of the shield;
(6) Size and shape of the orbital openings;
(7) Shape and extension of the lateral and dorsal fields;
(8) Ornamentation.

Comparison of the criteria used in establishing species of *Trematapis* gives the present author some qualms, since only one species recognized as valid in my 1938 paper had been based on material other than that of the Patten Collection at Dartmouth College. Patten had erected but very inadequately characterized two species, *T. milleri* and *T. mammillata* (1931), so that, in addition to naming and describing four new species, my paper was the first actual description of the two erected by Patten. Thus the problem facing me here is the discussion of the criteria I used at that time and the attempt to correlate this with the criteria recognized in the above list for the cephalaspids.

Unfortunately only one species has been recognized in the genus *Oeselaspis* and only one in the genus *Dartmuthia*. The same is true of the genera *Saaremaaspis* and *Rotsiküllaspis*. Two species of *Witaaspis* were recognized (Robertson, 1945). They were distinguished on the basis of ornamentation, including the form of the median dorsal crista.

The specific criteria used for the species of *Trematapis* were as follows:

(1) Form and proportions of the shield;
(2) Form of the naso-hypophysial fossa and relation of the aperture to the fossa;
(3) Form of the median dorsal crista of the shield;
(4) Ornamentation of the shield;
(5) Microscopic appearance of the superficial layer of the shield, especially the distribution of the fine "pores."

There are certain fundamental differences in shield plan in the *Trematapidae* from that in the *Cephalaspidae* which make some of the cephalaspid criteria unusable for trematapids. The rostral margins of the shields are similar throughout in the trematapids. Cornua, pectoral sinuses, and interzonal region are absent in trematapids, correlated with the greater

extent posteriorly of the shield. Dorsal and lateral fields are in general very uniform, although in *T. patteni* the form of the anterior lateral field differs from that found in any of the other species.

The criterion of specific distinction I used was difference in one or more of these characteristics. Conversely, identity of species was based on similarity in all the diagnostic features.

On the basis of shield proportions the seven species could be divided into two groups, one including *T. schmidti*, *T. rohoni*, *T. scalaris*, and *T. panderi*, the other including *T. milleri*, *T. mammillata*, and *T. patteni*, although the distinctly larger size of *T. milleri* might be used as a basis for placing it in a separate group.

On the basis of form of the naso-hypophysial fossa and aperture *T. schmidti*, *T. milleri*, *T. rohoni*, and *T. patteni* fall into one group, *T. mammillata* into another. This feature is not adequately preserved on the specimens of the other species.

The form of the crista appears to be different on each of the seven species, although the preservation of that region on the specimen of *T. scalaris* is not good.

On the basis of the ornamentation of the shield, chiefly the pattern of tubercle distribution, *T. milleri* resembles *T. schmidti*, although there are fairly definite differences. *T. patteni* and *T. mammillata* are similar in this, with the exception of the thinness of the *T. patteni* shield, which was one of the major reasons for Denison's decision (1947) that it might be a juvenile stage of the other species. *T. scalaris* and *T. panderi* resemble *T. mammillata* in tubercle distribution, but each has a peculiar, "ladder-like" marking on the dorsal shield surface which I used as the differentiating criterion.

The microscopic appearance of the shield surface, especially the distribution of the fine "pores," was a final character used. On this basis *T. schmidti*, *T. milleri*, and *T. rohoni* were definitely distinguishable as separate forms. *T. patteni*, *T. scalaris*, and *T. panderi* resembled *T. mammillata*.

In 1947 Denison stated that *T. patteni* was based on immature specimens of *T. mammillata*, and that *T. scalaris* and *T. panderi* were based on specimens of *T. mammillata* showing growth anomalies which accounted for the "ladder-like" orna-

mentation. His major criterion for conspecificity was the microscopic (histologic) structure of the bony shield, *i.e.* the last character I listed above.

I am willing to concede that Denison's contention might be correct, that *T. patteni* may be based on immature specimens, the shield characters which superficially seem to distinguish it from other species being due to immaturity. However, until additional specimens definitely establish the validity of his contention I believe it wiser to retain the species. There were only a few specimens of this type in the collection. Moreover on the basis of the other characters listed above there is doubt as to its identity with *T. mammillata*. It resembles this species in pore distribution, distribution of the dorsal tubercles, and shield proportions; differing from it in the form of the nasal fossa, in which it is more like *T. schmidti*, in the form of the dorsal crista, and in the emargination of the anterior lateral field. In a foot-note (1947, p. 358) Denison discounted these differences on the basis of the thinness and poor preservation of the shields.

The crucial point on this question is whether identity in histologic structure of the bony exoskeleton is a valid criterion of conspecificity. In his paper Denison states (p. 337): "It was found that the microstructure of the exoskeleton, viewed either superficially under xylol or in thin sections, offered clear characters which sufficed for a *specific identification* of even a small fragment of *T. mammillata*, *T. milleri*, *T. schmidti*, and *T. rohoni*. The other species described by Robertson were shown to belong to *T. mammillata*." It is on the basis of this microstructure of the bone and the extreme thinnness of the exoskeleton of the specimens referred to *T. patteni* that Denison regarded them as being simply juvenile specimens of *T. mammillata*.

Denison's hypothesis, that "the exoskeleton was formed only after the attainment of full growth" (p. 362) and that these specimens of *T. patteni* offer conclusive proof of the hypothesis, is attractive, since many of us who have worked with the ostracoderms have been puzzled over growth methods. It is important, however, to be more certain of this before the conclusion can be accepted.

My use of the microstructure of the shield as a taxonomic

character (1938) perhaps needs explanation if I am to contend that Denison's union of species on its basis is not justified. I have never regarded histological characters as adequate basis for specific *identification.* Rather it was a character in which there were definite differences in pattern, and I used it as an additional criterion for *separation* of species. If histology of the bone were to be used as a basis for union of vertebrates at the specific level I suspect that we would very markedly reduce the number of species. Certainly I would regard it as a totally different grade of organization than the characters listed earlier for cephalaspids.

The case against the other species, *T. scalaris* and *T. panderi*, is somewhat different, although here again Denison has used the microstructure of the shield as the basis for uniting them with *T. mammillata.* When I first found the specimens which I later separated as *T. scalaris* and *T. panderi* I considered the question of anomalous characters. My reason for rejecting that explanation of the ladder-like markings was the symmetry of these markings. They may be related to the pores and polygonally-arranged canals ("sensory canals" of Denison), but that in itself does not indicate that the difference from the usual pattern is simply due to "growth deficiency in these specimens." It is not demonstrated that these peculiar markings might not have been permanent markings of a variant form rather than anomalous characters of *T. mammillata.* Had they shown less symmetry of pattern; had they been unilateral or irregularly-arranged markings on the shield, I would not regard them as taxonomically significant.

Denison's contentions regarding these three species may be correct, but it still appears to me to be wiser to retain the independence of distinguishable forms until we can be somewhat more certain that they represent either juvenile specimens or anomalous specimens of another species.

I wish here to express a doubt regarding the conclusion that the system of "mucous canals" in the skeleton of *Trematapsis* was a part of the "sensory canal system." Denison's careful work on the exoskeletal structure (1947) seemed to indicate that the grooves of the lateral line system followed lines of these canals and apparently had openings into them. That does not necessarily mean that they were parts of a functional

whole. I puzzled a good bit over the pathway of innervation of the lateral line canals in *Trematapsis* and had come to the conclusion (1938, pp. 199-200) that the pathway through the exoskeleton must be by way of these pore canals, but I know of no morphological or physiological reason why the fine nerve branches might not make their way via these canals at the same time that the canals performed additional functions of their own, such as, perhaps, mucous secretion. I stated (p. 199) regarding the relation of the sensory canal grooves to the exoskeleton that there were "at least occasional apertures in the bottoms of grooves. These apertures prove, on dissection, to connect with the system of channels which pervades the bony shield in the same way as do the pores so abundant over the general surface." I then quoted from Stensiö (1927) that he had found on *Trematapsis* "branches from the vascular system opening into the mucous canals on the basal side, a state of things which shows that the mucous canals received vessels and nerves through the vascular canal system," and then commented: "Possibly this is the solution of the innervation difficulty. At least for the present no other appears available."

My major reason for doubting the sensory character of this intricate network is perhaps teleological. Denison states (1947, pp. 350-52): "It thus appears that the Silurian *Trematapsis* had its body entirely covered, even on the scale-covered tail, with a network of canals whose function was to receive vibratory stimuli. Such a generalized system of surface receptors may have been the primitive arrangement in *Osteostraci*, and indeed in vertebrates. We can see in *Trematapsis* the beginnings of specialization of parts of the canal system into what is known as the lateral line system of later fishes. This involves the assumption of a linear arrangement of the canals in certain regions, the acquisition of a more open connection with the exterior, and presumably the enlargement and specialization of the nerve branches which supply the definitive lateral line canals."

To me it does not make sense. I realize that the teleological argument is frowned upon by biologists, but I cannot resist in this case asking why a semi-benthonic, weak-swimming creature like *Trematapsis* should have been so extravagantly blessed with receptors. The canals in question may have served to

secrete mucus or may have been used for some other function, but the fact that they indicate possible pathways for the nerve fibres whose endings innervated the sensory canals of the lateralis system does not seem to me to justify the assumption that they themselves constituted "sensory canals."

In summary, it would appear possible in the *Osteostraci*, in so far as those groups are concerned which contain more than single species, to use specific criteria of what seem to be comparable "grades of organization," although the detailed characters on which specific distinctness is based in one genus cannot be carried over to all other genera within the order, and sometimes cannot be used for different genera within the same family. This was to be expected on the basis of what we know of taxonomic work in general and what we believe to be the nature of the evolutionary processes by which species arise.

The class *Ostracodermi* differs sufficiently from other vertebrates that it seems justifiable to place it in a super-class, *Agnatha* (Robertson, 1942, pp. 147-149), characterized by lack of true vertebrate jaws and by gill pouches of the cyclostome type, characters which have led to the union of the modern cyclostomes with the fossil ostracoderms.

The class has been subdivided into two sub-classes, *Pteraspidomorphi* and *Cephalaspidomorphi*, on the basis of type of exoskeletal pattern and apparent differences in the relation of the individual branchial apertures to one another, the order *Osteostraci* constituting the best-known group of the cephalaspidomorphs, differentiated from the other order or orders, dependent on the detailed classification adopted, by the type of shield structure.

Familial characters of this order (Robertson, 1945, p. 186) include form and extent of the shield, presence or absence of cornua and the associated pectoral sinuses, number of lateral fields, extent of dorsal field, and size and number of oralobranchial plates.

Sub-families have been recognized only in the *Cephalaspidae* and are based on characters to some extent intermediate between those used for family differentiation and those characterizing genera, namely, the extent of the cornua and pectoral sinuses, size of the dorsal and lateral fields, and, in

Stensiö's differentiations (1927), differences in distribution of nerves to the lateral fields.

Generic distinctions are of importance chiefly in the cephalaspids, since the other families, with the exception of the *Dartmuthiidae*, are as yet based on single genera. *Rotsiküllaspis* (Robertson, 1938, pp. 490-493; 1945, p. 190) has been placed in the *Dartmuthiidae*, the generic differences being conformation of the posterior margin of the shield, type of ornamentation, extent of the lateral fields, and form and arrangement of the oralo-branchial plates. Romer (1945, p, 572) concurs in this placement of *Rotsiküllaspis*, but also lists *Didymaspis* in this family, as mentioned earlier in this paper. I am inclined, however, toward disposition of this genus (1945, p. 789) as a separate family. The possession of rudimentary pectoral sinuses, the peculiar form of the lateral fields, and the general resemblance in form to *Oeselaspis* are the major reasons for this decision.

The series of distinctions listed for genera within the *Dartmuthiidae* does not differ materially in what I have termed "grade of organization" from those used for cephalaspid genera, namely, general form of the cornua and pectoral sinuses, extent of the dorsal and lateral fields, extent of the shield, and relation of the dorsal shield to the ventral armor.

Specific criteria have been discussed earlier in this paper.

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NOTE ON SURVIVAL OF FABRIC CHARACTERS IN YULE MARBLE AFTER HEATING TO 700° C.*

FRANCIS J. TURNER AND C. S. CH'IH
(with comments by Joseph L. Rosenholtz and Dudley T. Smith)

IN 1948 J. L. Rosenholtz and D. T. Smith of Rensselaer Polytechnic Institute determined linear thermal expansion of the marble from Yule quarry, Colorado, that has been the subject of high-pressure deformation experiments by D. Griggs and of detailed petrofabric analysis by E. B. Knopf and F. J. Turner. (For an account of this work and a full statement of the properties of Yule marble, see Knopf, 1949.) The thermal expansion tests were carried out on three oriented, mutually perpendicular rods, cut from the same block as was used for detailed petrofabric analysis by Turner (1949). These rods may be referred to as the P, Q and R rods, respectively normal to the faces P, Q and R of the parent block. [P rod is elongated N-S, horizontal, parallel to foliation; Q is E-W, horizontal, normal to foliation; R is vertical, parallel to foliation.]

The maximum coefficient of thermal expansion, that measured in rod Q, was found to be greatly in excess of that for a single crystal of calcite. Moreover, notable permanent expansion of all three rods was recorded at the end of a single cycle of heating to 700° C. and subsequent cooling to room temperature. A further heating-cooling cycle showed no such effect, however, the expansion on heating being followed by equivalent contraction on cooling through the same interval of temperature. It would seem that in the first cycle of heating some internal structural adjustment occurs, which results in permanent relief of a strain present in the untreated marble. With a view to determining to what extent such relief of strain might be reflected in the fabric, the writers carried out a detailed petrofabric analysis on a longitudinal section cut from the thermally expanded rod Q, and oriented parallel to plane R, using the same technique as had previously been employed in investigating the fabric of the untreated rock (Turner, 1949).

* This paper embodies results of research sponsored by the Institute of Geophysics, University of California.

The fabric data were compared carefully and scanned for possible evidence of recrystallization, change in preferred orientation of calcite grains, and change in orientation, number or closeness of spacing of glide lamellae. In every respect the two fabrics were found to be almost identical. This conclusion is unexpected enough to warrant recording the essential data.

Yule marble is an even-grained rock. But sections of the untreated marble show a small percentage (by area) of notably small grains whose diameter is one-third to one-fifth that of the predominant type of grain. In the single section of expanded marble, small grains are less numerous; but the degree of dissimilarity is of the same order as the variation observed between different parts of one large section of untreated marble. There is thus at most only a very inconclusive indication that some of the smaller grains may have been eliminated by recrystallization during the heating test. The writers would disregard this evidence completely.

Figures 1, 2 and 3 are orientation diagrams prepared from optical and crystallographic measurements made on 100 typical calcite grains in the thermally expanded marble. They show the preferred orientation pattern for c axes (fig. 1), observed $\{01\bar{1}2\}$ lamellae (fig. 2), and edges $[e:e']$ between observed $\{01\bar{1}2\}$ lamellae in individual grains.[1] These may be compared

[1] More than half the measured grains show two sets of $\{01\bar{1}2\}$ lamellae only. In grains where three sets are present, one is almost always much weaker than the others; this weak set was omitted when plotting edges of the $[e:e']$ type—just as was done in the previous investigation.

Figures 1-4. Preferred orientation of calcite in Yule marble after heating to 700° C., based on measurements on 100 grains in a section (cut parallel to plane R). Inset figure gives geographic orientation of diagrams (lower hemisphere of projection, looking vertically downward).

Fig. 1. 100 c-axes, [0001]. Contours at 6%, 4%, 2%, 1%, per 1% area; maximum concentration 10%.

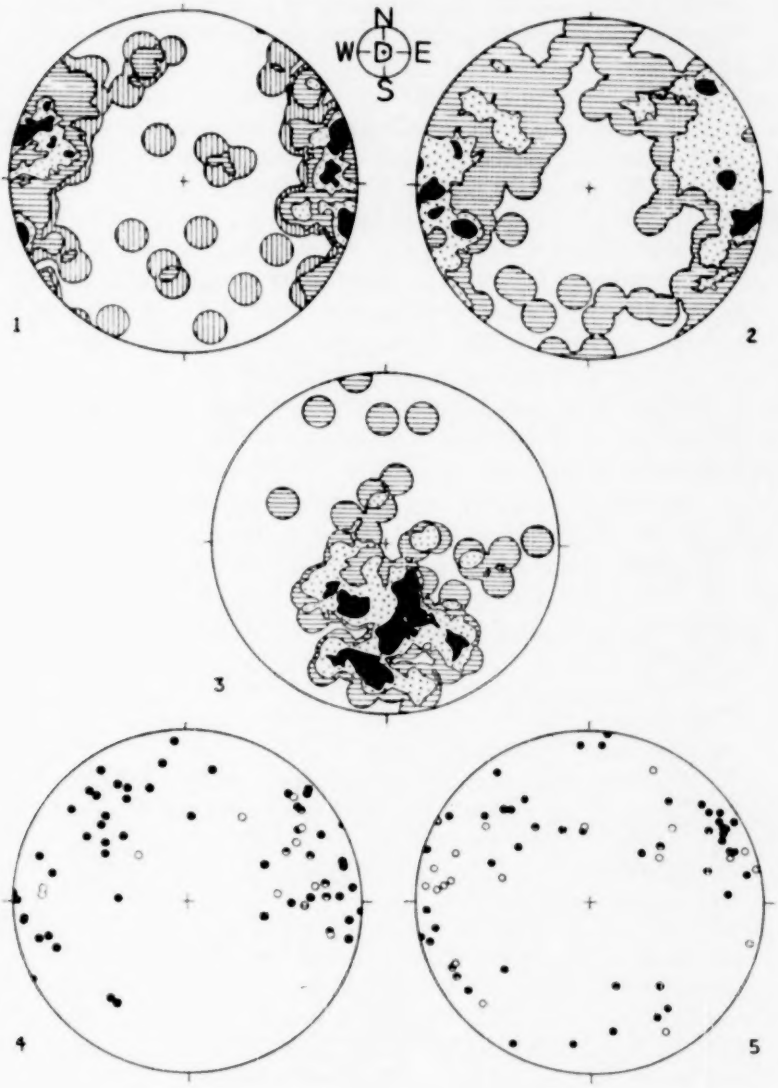
Fig. 2. 191 poles of $\{01\bar{1}2\}$ lamellae. Contours at 4%, 2%, 0.5%, per 1% area; maximum concentration, 5%.

Fig. 3. 82 edges $[e:e']$ between faces of $\{01\bar{1}2\}$. Contours at 5%, 3%, 1.1% per 1% area; maximum concentration, 7%.

Fig. 4. Poles of $\{01\bar{1}2\}$ lamellae recognizably twinned (solid circles); poles of $\{01\bar{1}2\}$ lamellae with spacing index > 20 (open circles).

Figure 5. Yule marble, *untreated*, section cut parallel to plane R. Poles of $\{01\bar{1}2\}$ lamellae recognizably twinned (solid circles); poles of $\{01\bar{1}2\}$ lamellae with spacing index > 20 (open circles).

with figures 8, 12 and 28 respectively in the previous paper (Turner, 1949, pp. 603, 605, 611). Corresponding diagrams for treated and untreated marble are so closely similar that the fabrics must be regarded as identical. This similarity applies also



to diagrams constructed for other crystallographic directions such as edges between $\{10\bar{1}1\}$ and $\{01\bar{1}2\}$. The preferred orientation patterns of the crystal lattices of calcite—as illustrated by the optic axes (fig. 1) and the visible deformation lamellae (figs. 2, 3)— have survived heating and cooling through two complete cycles without the smallest recognizable modification.

It was thought possible that the nature or closeness of spacing of deformation lamellae might have been changed to some extent by heating. To test this, counts were made of the number of sets of lamellae in which twinning could be recognized definitely (in one lamella or more) by symmetrical extinction when viewed parallel to the plane of the lamella between crossed Nicols. The number of continuous lamellae in each set, the number of these recognizably due to twinning, and the maximum width of the grain normal to the plane of the lamellae in question were also measured.

Similar counts and measurements were made for 100 grains (excluding small interstitial granules) in the corresponding section of untreated marble. The results obtained are as follows:

(1) The number of sets of $\{01\bar{1}2\}$ lamellae observed per 100 grains in untreated marble is 194; and of these 47 (24%) show one or more recognizably twinned lamellae. Corresponding figures for thermally expanded marble are 182 sets, 52 (= 28%) with recognizably twinned lamellae.

(2) In the untreated marble 13 sets of lamellae include one or more broad twinned bands; none such appears in the thermally expanded rock.

(3) The total number of individual $\{01\bar{1}2\}$ lamellae observed in all sets in the untreated marble is 912, of which 107 (12%) are definitely twinned. Corresponding figures for thermally expanded marble are 735 lamellae: 79 (11%) definitely twinned.

The greater total number of lamellae observed in the untreated rock noted under (3) above is partly due to the somewhat larger size of the grains measured in that section. A more satisfactory index of the closeness of spacing of lamellae in any set is given by the expression

$$\frac{\text{number of lamellae in the set}}{\text{width of grain normal to plane of lamellae}}$$

This we have termed the spacing index. The mean spacing index for all sets of well-defined lamellae was found to be 11.1 (per mm.) in the untreated rock as against 10.0 in the expanded rock. These indices may be corrected to include a value of 0 for every $\{01\bar{1}2\}$ plane not represented by observable lamellae; these corrected values are 7.0 (untreated) and 6.7 (expanded). Statistically identical closeness of spacing of lamellae brought out by these indices may be contrasted with the much higher values of mean spacing indices computed for lamellae in 100 grains measured in Yule marble elongated 4% in tension at room temperature and confining pressure of 2830 atmospheres[2]: these are 45.9 and 42.2 respectively.

Figures 4 (thermally expanded marble) and 5 (untreated marble) give plots of poles of lamellae having spacing index of 20 or higher (open circles), and of lamellae that are definitely twinned (solid circles). The lamellae with high spacing index are evenly distributed in the lamella girdle of the untreated marble; but in the expanded marble they are restricted to a more limited sector—the area of maximum lamella concentration in the top right quadrant. On the other hand heating seems to have had no significant effect upon the distribution of those sets of lamellae which include recognizable twins.

Results of the investigation may be summed up as follows:

(1) Yule marble heated to 700°C. and cooled to room temperature has maintained its fabric characters in great detail. There is no recognizable significant change as regards mean grain size, orientation of space lattice of grains, orientation of observed $\{01\bar{1}2\}$ lamellae, orientation of definitely twinned lamellae, spacing of lamellae per unit width of grain, or percentage of lamellae recognizably due to twinning.

(2) In two minor details the fabric data indicate that thermal expansion of Yule marble may possibly have been accompanied by very slight recrystallization at loci of

[2] We gratefully acknowledge use of this section—supplied by Professor D. Griggs, Institute of Geophysics, University of California, from material now in progress of investigation.

maximum strain. Broad twin lamellae, appearing in 7% of the observed sets of lamellae in the unheated rock are absent in the heated specimen. They may have been eliminated or reduced to linear dimensions by heating. Although the mean lamella spacing index is virtually identical in the two specimens, it is found that whereas 16% of the sets of lamellae have an index of 20 or more in the unheated specimen; this value is reduced to 10% after heating; moreover the orientation of such sets with high index is much more restricted in the heated rock. It is possible that neither of these minor apparent modifications of fabric is real.

The writers are greatly indebted to Professors Rosenholtz and Smith for the material on which this paper is based and for permission to discuss certain of their as yet unpublished data on thermal expansion of Yule marble. The detailed fabric analysis of heated and unheated Yule marble is part of a series of investigations made possible by a research grant from the Institute of Geophysics of the University of California. This is gratefully acknowledged.

For References, see p. 354.

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COMMENTS ON THE CHARACTERISTICS OF HEAT-TREATED YULE MARBLE

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Studies which have been completed and others which are now in progress make it remarkably clear that fabric analysis and linear thermal expansion data may lead to the same conclusions concerning important aspects of the deformational history of rocks. The authors appreciate the invitation of Professor Turner and Miss Ch'ih to add their suggestions and comments here.

The coefficients of linear thermal expansion, α, of a deformed rock are not merely general handbook values; their analysis,

particularly when supplemented by fabric analysis, may be most revealing. In a comparison of the expansion of Yule marble with that of a single calcite crystal (Rosenholtz and Smith, 1949), it was found that structural changes are produced in the marble by heating to 700°C., as evidenced by the changes in α and, more particularly, by large permanent length recovery. Other studies on metals, which have not yet been completed, give conclusive evidence that compressive stresses above the yield point produce very large changes in α, as subsequently measured in the direction of compression. The equivalent of this condition was found in the Q rod of Yule marble, which is normal to the foliation and has an E-W horizontal geographic orientation.

Knopf (1949) and Turner (1949) have shown that approximately half of all grains examined in their fabric studies on Yule marble have their optic axes alined close to the E-W direction. Inasmuch as $\alpha\parallel$ for calcite is large and $\alpha\perp$ is both small and negative, a means is at hand for determining preferred orientation from expansion analysis. If it is assumed that all grains in Yule marble have their optic axes either parallel or normal to the E-W orientation, a *c* axis concentration of 39 to 49 percent parallel to E-W may be computed from the expansion data, depending upon the temperature range selected for computation. Turner found that 52% of 340 plotted *c* axes in untreated marble made angles varying from 55° to 90° with the plane of foliation. Since the heat-treated marble revealed no significant change in this respect, we may use either the treated or untreated marble for a grain by grain summation of $\alpha\parallel$ and $\alpha\perp$ computed from the individual *c* axis orientations. It is hoped that such a study may be made soon in order to establish the proper temperature range to be used in computing preferred orientational values.

The conclusion of Turner and Ch'ih that there is no appreciable change in fabric that can be attributed to crystallization after heating to 700°C. is important; it is probable, however, that incipient recrystallization does occur. The evidence from thermal analysis suggests that a slight change in the rate of change of α occurs in the vicinity of 350°C.; and for this no other explanation has been found. Turner and Ch'ih make a similar suggestion regarding apparent elimination of broad

twin lamellae and reduced percentage of sets of lamellae that have a spacing index of 20 or more. They do, however, reserve decision as to the reality of such changes. Certainly the changes in both fabric and expansion characteristics are small, but the fact that they are recognizable is important. Later work may reveal their full significance.

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DISCUSSION

ORIGIN OF PIMPLE MOUNDS, by E. L. KRINITZSKY

This paper, AMERICAN JOURNAL OF SCIENCE, vol. 247, pages 706-714, is one of many in which an author supposes that microrelief mounds of a single type may have different origins in different regions without change in essential characteristics except a variation from fine sand and silt to gravel and cobble in the coarser components of the material. In this case it is also, more or less tacitly, implied, without full consideration of details, that soil development and accompanying mass wasting will level such original differences as between streamlined, fine-grained and cross-bedded eolian sand dunes, sub-aqueous "dunes" in the form of river bars with moundy elements, and mounds made by rodents.

Although the reviewer believes that this paper was outdated when it appeared, it is desirable to discuss it, because it is typical of a large class in the literature and because it is instructive in several features. The revival of interest in the pocket gopher theory began with V. B. Scheffer's restatement (1947) and continued mostly in the "Scientific Monthly" in 1948 with the resurrection of F. C. Koons' highly significant report of his years of observation of the growth of pimple mounds in Texas by the agency of pocket gophers, and the discussion by several others of the definitive role of poor surface and underground drainage in leading the rodents to build small "winter mounds" to hold the nest and food-hoard above winter floods, resulting in the eventual development of pimple (or mima) mounds by repeated additions of soil thus brought to the nest area from locations always at the margin of the growing heap. The "winter mound" is a weak structure, commonly destroyed by animals or the weather by the time it is needed again after the first flooding.[1]

Krinitzsky has missed (1) the critical item of mound structure, *the bi-convex lens of top (A zone) soil*, forming a wholly abnormal thickening of the top soil, inexplicable under any origin except the sort of diligent underground cultivation done only by the pocket gopher, among burrowing animals of a size adequate to produce these mounds and to move gravel, (2) the present writer's statement that (Price, 1949) the bi-convex structure has been found to be characteristic of the mounds in all regions of occurrence of the type—nearly every state west of Mississippi River—and (3) that the mounds do occur abundantly (*contra* Krinitzsky, p. 712) where gophers live in thin "residual" soil above hard rocks and soil

[1] Full documentation of this discussion is not attempted, being covered in the papers cited here and by Krinitzsky.

claypan. The rocks include basalt flows and eolian pavements and the claypans occur in California and also in the Gulf Coast, where Krinitzsky seems not to have seen them under mounds. He also misses entirely the later development of the gopher theory in which a "shallow lying base of gravels" is found to be by no means an "integral requirement of the gopher-hypothesis" (p. 712), any feature denying rainfall a chance to drain away being sufficient.

Krinitzsky seems not to have studied the soil zonation of pimple mounds widely, as he says that the material of the mounds is "entirely distinct from surrounding soils" (p. 706), a statement not borne out by careful examination of the soil in areas where mounds are freshly developing, but true in part where the gophers have completed their task of collecting all the available top soil into mounds. Even then, there are similarities, and in early stages almost identities, between the B and C zones and claypan and hardpan layers of the mound and inter-mound areas.

Krinitzsky has been misled by the excellent preservation of Prairie (younger Beaumont) microrelief topographic grain (plates 1, 3, 4) into thinking that pimple mounds are only slightly altered features of Pleistocene age. He has missed the important unconformable relationship of pimple (mima) mounds with the Pleistocene and some Recent deposits. The mounds are far more perfectly preserved than are most similar microrelief features of small size and loose, sandy composition but of Pleistocene age. They also occur in vast numbers on worn-down distributary ridges (natural levees) of Montgomery (older Beaumont) and some Prairie deltaic plains in Texas and Louisiana, where from 10 to 25 feet of height of the levees has disappeared. They also occur in numbers on Pleistocene depositional surfaces being distributed in relation to modern surface features imposed on them, such as rain ponds and shallow drainage swales of recent origin.

Krinitzsky misses in the literature the highly significant relationship of mima mounds to gully divides. These small ridges are thickly studded with mima mounds in parts of most states where the mounds are known. Such divides range in age from dissected, inactive Pleistocene features of dissected alluvial fans along the eastern side of the Central Valley of California, to gully divides now in growth on rolling hillsides of eastern Texas and adjacent Louisiana. The mounds widen the divides, interfering with the normally straight courses of the gullies and producing entirely abnormal sinuosities. These same distinctive types of fan-shaped or radiating gullies of alluvial fans and rounded hill tops occur

[a] "Recent" is retained for the Gulf Coast, where detailed correlation with the glacial section is not yet accomplished.

abundantly in states east of the Mississippi River, where the writer has examined them from the air lanes, but without anywhere showing any tendency to develop the sinuosities of the mound-occupied gullies. Reticulated gully systems do not occur along the air lanes of the East, so far as the present writer has been able to observe, with any regular system of mound-formation of mima type such as would be required by the hypothesis of F. A. Melton (1935). Surely processes producing natural mounds west of this river should operate under similar circumstances also east of it, where gophers are absent.

While existing mima mounds are believed to be of late Recent origin, it is possible that there were Pleistocene mima mounds and even that individual mound colonies have remained *in situ* and under occupation by gophers since then. However, the amount of reduction of sandy surfaces during the development of mature soils is commonly appreciable and we know that some of the Pleistocene depositional plains have minor surfaces of erosion on them. This subject is speculative. If Pleistocene mima mounds occur, the rodents must have kept them in repair during long periods since then. Their habit of decorating the mound surface with numerous small piles of soil keeps existing mounds fresh and relatively smooth-contoured.

With regard to the "eolian dune" hypothesis, Krinitzsky assumes that the mima mounds seen in plate 4 abundantly dotting beach ridges and parts of the abandoned lagoonal plain of the Ingleside Pleistocene shoreline terrace at Smith Point, near Galveston, Texas, formed as small mound-shaped dunes on beach ridges. The writer has an intimate familiarity with the barrier islands of the western coast of the Gulf of Mexico from southwestern Louisiana through Texas to Tampico, Mexico. Nowhere is such a relationship found in active beach ridges, although impermanent low shrub-coppice mounds form on the bare upper parts of active beaches, but without being preserved on the older, stabilized ridges. The habit of dune formation on barrier beaches of this coast is as follows:

In the humid and moist sub-humid regions, the Recent and Pleistocene beach ridges of prograded beach plains are low, regular, relatively even-topped and without dunes, except as stated above. In the dry sub-humid and parts of the semi-arid regions, eolian sand loads the vegetation of the beach ridges, building up smooth, wide foredune ridges that rise above and cover two or more beach ridges with the intervening swales. The ridges do not support other dunes upon them and blowouts are rare. Both conditions are found on the Recent barrier island between Galveston and Corpus Christi, pimple mounds being absent.

Where sand supply is large and moves actively in the wind, as is here the case only in the semi-arid region, foredunes rise to maximum heights and blow out where over-steepened. U-dunes, elongate blowout dunes, amputated lag-ridge arms and dunes irregularly eroded by storm waves all occur stabilized, but without anything of pimple mound type. The beach ridges are obliterated here and the resulting topographic pattern is irregular and of a much larger grain than pimple mounds. It would not yield a pimple-mound type of topography on weathering. On the Pleistocene barrier islands of the coast in this semi-arid region, the islands are built up 15 to 20 feet or more higher (than the sections in the humid region) by eolian materials and their surfaces are old dune plains of smooth contours. The dune remnants are broad and indistinct, virtually indeterminate, the plain being dominated by transverse blowout swales somewhat like broad, open yardang grooves.

The somewhat reticulated microrelief of low sandy ridges with mound-like portions and isolated ovals and mounds shown in plate 2 and said to have formed as sub-aqueous "dunes" in Mississippi River makes a welcome contribution to the knowledge of sub-aqueous microrelief and merits still further detailed illustration besides that shown in plate 5. If such relief is preserved in gopher-occupied areas it would doubtless be quickly occupied by the rodents and, if drainage were poor, might become decorated by mima mound development. However, unless mima mounds were developed here, there would be no formation of bi-convex lenses of top soil. *Without such soil structure, no mounds, even if of suitable size and distribution, belong to the category mima, or pimple.* This is a feature that must be taken into account by the proponents of polygenetic origin for mima mounds. Unfortunately, most geologists are not versed in the fundamentals of soil science and are unfamiliar with the field relationships of soil zones.

The listing of pimple mounds in floodplains in Tennessee by Krinitzsky (p. 707) merits further investigation, as this is the first record from a state that extends east of the Mississippi River (Price, 1946, figs. 1 and 2).

The mound-like or circular mottles of the glacial swell and swale described by Gwynne (1942) cited by Krinitzsky (p. 710), from Iowa seem to be of a coarser grain than mima mounds, the diameters of the supposed mounds ranging up to 600 feet. If these are the mottles of leveled ridges and mounds, it is strange that no unleveled areas remain. Mima mound mottles on leveled farm lands have the same appearance from the air, but seem to be smaller.

The present knowledge of mima mounds is the result of a thorough exchange of views and information between research workers

and field surveyors working in rodent control, soil structure and history, drainage and groundwater, and geomorphology. Little contribution to interpretation was made by the greater number of the papers published before W. W. Dalquest and V. B. Scheffer announced their findings with regard to the close relation between pocket gophers and mima mounds. Only M. R. Campbell (1906) among the early workers saw that organic deposition was necessary to explain the abnormal thickening of the top soil in the mounds. Many writers have evidently written as though the mounds were a mere scientific curiosity and have treated them lightly, rushing to give a "solution" on the basis of hearsay, examination of photographs, or external examination of mounds in the field. When, as in the present case, an author has hit upon a seemingly satisfactory analogy for the area that he knows best, too often he has been content to let it go at that, resolving the difficulties raised by distant mounds by assuming polygenetic origin. Polygenetic origin for identical features even of this simplicity must be rare indeed.

In this article and the present writer's discussion of it, two cases of the double use of a common geomorphic term between geologists and hydrographers or oceanographers is evident. "Dune" is used interchangeably for eolian and sub-aqueous features by hydrographers, following Gilbert. Geologists now must write "barrier island" instead of Douglas Johnson's "offshore bar" because oceanographers use the latter term in its original, simple meaning of a submarine bar. Confusion has arisen in both cases, neither of which should have occurred.

The writer is in harmony with Krinitzsky's discussion of hypotheses of pimple mound origin other than those noted in the foregoing paragraphs (Price, 1949). W. ARMSTRONG PRICE

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REPLY

In proposing a polygenetic theory of origin for pimple mounds the writer has not dismissed gophers as being builders of these features. However, their universal importance has been brought into question.

The natural mounds referred to in western Tennessee are the "sand blows" described by M. L. Fuller (U.S. Geol. Survey Bull. 494, Washington, 1912) as having originated by extrusion during the famous New Madrid earthquake of 1812-13. Fuller distinguishes these features from mounds of other origin.

Price in his discussion brings out a very important point. He states that rodents develop a bi-convex lens of top-soil and that "*Without such soil structure, no mounds, even if of suitable size and distribution, belong to the category mima, or pimple.*" Hence Price makes it a matter of definition that internal characteristics as well as surface appearances be considered before a mound may be classed as a "pimple." If this position is taken, certain natural mounds would be excluded from this category. Also mounds of other origins, but which later have been occupied and partly preserved by gophers, would not be interpreted through their original genesis but solely by their gopher history.

Perhaps it is desirable to refine definitions by setting close limitations. If a definition of this sort becomes accepted, it must be admitted that some differences on the origin question would be clarified.

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REVIEWS

Principles of Electricity and Magnetism; by G. P. HARNWELL. Pp. xvi, 670. New York, 1949 (McGraw-Hill Book Co., Inc., $6.00).—This is the second edition of a standard text used in upper division courses on electricity and magnetism. First published in 1938, it has won its way among text books because of its comprehensiveness and its full treatment of applications. The new edition preserves the advantages of the old but brings the contents up to date.

On the whole the new book accentuates the subjects of electronics and high-frequency radiation, and it does justice to the new developments in accelerator research. Specifically, the following sections have been added to the older text:

Forces on conductors and dielectrics
Electron structure of crystals
Magnetic induction accelerators
Feedback amplifiers
Very high-frequency oscillators
Propagation of electro-magnetic waves in metallic enclosures

The size of the book has grown from 619 to 670 pages.

HENRY MARGENAU

Heat Transfer, Vol. I; by MAX JAKOB. Pp. xxix, 758; numerous figs. New York, 1949 (John Wiley & Sons, Inc., $12.00).—This is an excellent advanced text and reference volume covering all aspects of the subject of heat transfer. A good knowledge of the calculus and differential equations is necessary for the understanding of this book, but the treatment is not highly mathematical. Considerable space is devoted to the explanation of the physical properties of matter that play a role in heat transfer, and there are well-chosen admixtures of empirical material and descriptions of experimental methods and techniques.

The value of this book as a reference work is enhanced by the unusually detailed and logically-ordered table of contents, as well as by the fairly complete alphabetically-arranged bibliography. The author obviously has a full knowledge of both the fundamental physics and the engineering applications of heat transfer, and both are equally well presented. W. W. WATSON

Biochemical Preparations, Vol. I; by H. E. CARTER (Editor-in-Chief), E. BALL, C. NIEMANN, R. R. SEALOCK and E. E. SNELL. Pp. xi, 76. New York, 1949 (John Wiley and Sons, Inc., $2.50).—This is the first volume to appear in a prospective series designed

to provide authoritative and thoroughly checked preparations used in biochemical research. It is hoped that this series will achieve for biochemistry what *Organic Syntheses* has done for organic chemistry. To further this purpose, the editors of *Biochemical Preparations* have conferred with the editors of *Organic Syntheses* in order to avoid those mistakes which are frequently made in starting a new series. It is the intention of the editors to publish a new volume every 12 to 16 months.

This series will cover the preparation of substances which are not readily available commercially, but which can be prepared from available starting materials. Isolation methods from natural materials will be emphasized, but where synthetic methods are more suitable these will be given. In addition, techniques of general application in preparative work will be included.

The present volume details sixteen preparations of which four are synthetic, *e.g.*, *p*-hydroxyazobenzene-*p'*-sulfonic acid and DL-glyceraldehyde 3-phosphoric acid. Of the other preparations six are for various amino acids such as L-alanine, L-glutamine and D-tyrosine, and three are for nucleic acids (adenosine triphosphate, diphosphopyridine nucleotide). In addition the isolation of the carotenoid lycopene from tomato paste and the preparation of crystalline lysozyme (globulin G_2) from egg white is given as well as several less readily classified substances.

The reviewer has found the presentation of the preparations to be remarkably lucid, precise and complete. Expected yields, conditions, manipulative techniques, explanatory foot-notes and references to the original literature are given. The edition is singularly free of typographical errors and is well indexed.

ALFRED H. FRYE

Geology of the Sudan; by GERALD ANDREW; chapter VI (pp. 84–128) of *Agriculture in the Sudan*, edited by J. D. Tothill. Oxford, 1948 (Oxford University Press, 42 shillings).—Geologists should know of this clear and concise summary of present knowledge of the geology of the Anglo-Egyptian Sudan. What is known of the basement complex and of the overlying flat-lying largely arenaceous sedimentary rocks (Carboniferous? to Cretaceous) is indicated, but special emphasis is placed on the late Cenozoic deposits, on recent processes of erosion and deposition, and on soils and soil formation. The description of the sediments in and around the semi-arid basin in the southern part of the country (swamp and flood-plain clay, dune sand, lateritic ironstone and related ferricrete sandstone and grit) will be of interest to students of continental sedimentation.

JOHN RODGERS

The Snellius Expedition: Vol. V, Geological Results; Part 3, Bottom Samples; by G. A. NEEB and P. H. KUENEN. Pp. 268; 16 plates, 33 figs. Leiden, 1943 (E. J. Brill, 30 guilders (appr. $8.00)). —This publication has been available in America since the war, but in view of its importance a belated review seems in order.

The report presents the results of the study of some 340 bottom samples taken in 1929-1930 from the Dutch vessel Willebrord Snellius in the eastern part of the Netherlands Indies. In this part of the archipelago, many large and small islands stand on ridges separated by basins 2 to 4 miles deep; in other words the area is neither strictly continental nor strictly oceanic. To many geologists, it is a modern counterpart of the "real" (eu-) geosynclines of the past.

In an introductory section, Dr. Kuenen describes the taking of the samples and discusses several topics of general interest, notably the statistical correlations found between organic matter, calcium carbonate, depth, and distance from shore. In the main body of the report Miss Neeb describes and classifies the samples and discusses the source and deposition of the bottom sediments. The principal types recognized are volcanic mud, terrigenous mud (derived from several types of terrain), mixed volcanic and terrigenous mud, coral mud and sand, and *Globigerina* ooze. Typical oceanic red clay is recorded from the Mindanao trough at the edge of the Pacific basin, not from the basins within the archipelago.

The data are summarized on an admirable map (scale 1:4,000,000) showing the distribution of the deep-water deposits. The distribution pattern is complex and is controlled not by a single factor such as depth or distance from shore but by the interaction of several, primarily the proportions of different materials supplied (volcanic ash, debris from the islands and from coral reefs, and shells of pelagic organisms) and the marine currents, secondarily the configuration of the basins, especially their relative isolation, the depth, and the distance from the source of the materials.

To a geologist of the future, the present sediments would display rapid and bewildering facies changes. Not improbably he would recognize resemblances to the rocks of at least certain pre-Recent geosynclines, such as the Alpine, though the pattern would perhaps be less linear. To geologists of the present, Dr. Kuenen and Miss Neeb have made available an excellent discussion of such rocks in the making.

JOHN RODGERS

Memoir on the Systems Formed by Points Regularly Distributed on a Plane or in Space; by A. BRAVAIS. Journal de l'Ecole Polytechnique, Cahier 33, Tome XIX, pp. 1-128, 1850. Translated by

AMOS J. SHALER, Massachusetts Institute of Technology, 1949. Memoir No. 1 of The Crystallographic Society of America ($3.90). Pp. viii + 114; 42 figs.—In this work Bravais demonstrates the properties of space lattices and plane lattices (nets). He leads logically to the 14 well-known Bravais lattices, and in the last section, polar or reciprocal lattices are defined and their properties developed.

This memoir is an important part of the foundation upon which modern crystallographic theory now firmly rests; it should be known to every crystallographer, be he interested in crystal morphology or structure, in scientific philosophy or in engineering applications. An early introduction to this work should therefore be a part of every student's course, for it will inevitably lead him to a better understanding and appreciation of the subject, and hence to more fruitful research and applications. That Bravais himself realized the application of lattice theory to crystals is shown in his concluding remarks, wherein he refers to this work as forming, "as it were, the preface of the theory of crystallography . . . " He surmised that a molecule or group of molecules would be associated with each lattice point, exactly as Laue's discovery proved 65 years later.

Bravais' presentation of his thesis in the shape of definitions, theorems, problems, corollaries, and scholia is formal, but the book is in fact surprisingly readable, and the method of presentation simply adds unusual flavor for modern readers. A few readers will find this feature annoying at first, but with a little practice, the form is as easily readable as any other for the presentation of the subject.

Mr. Shaler's translation not only gives English readers the content of this otherwise rarely available publication, but even captures some of the detached scientific spirit in which Bravais and his contemporaries usually wrote. HORACE WINCHELL

Science and the Moral Life; by MAX OTTO. Pp. 192. New York, 1949 (A Mentor Book, paper, $.35).—In his brief preface to this book, E. C. Lindeman of Columbia University characterizes Max Otto, professor emeritus of the University of Wisconsin, as "the 'homespun' philosopher." The expression is not wholly apt in this case because it has come to mean men like Will Rogers, often excellent, even great, in their way but men whose main talent is for the witty expression of the obvious or for well-turned support of popular prejudices. On rare occasions, Otto does skirt this field of "homespun philosophy" (perhaps in "With All Our Learning" or in "The Hunger for Cosmic Support" among essays in this volume), but

as a rule he is antipathetic to it. Lindeman's point is that Otto is not a speculative or academic philosopher but is one who expresses philosophy in terms of human actions and problems. His message is not banal and it is far from a mere restatement of folk beliefs.

The message is one of great concern to scientists and to the world. As expressed in one of these essays, Otto's view is that science, as historically developed in isolation from human emotional problems and with a tradition of "ethical neutrality," tends to destroy mankind. The alternative that mankind should, before it is too late, destroy science cannot be seriously considered. Otto's solution, in this particular essay ("Scientific Method and the Good Life") is "untrammeled study of fact in union with the hunt for the most promising means of general happiness." His pessimistic view of science here and more particularly in the most gloomy of the essays, "The Ethical Neutrality of Science," seems to be based almost wholly on the physical sciences. Elsewhere, more clearly than in the passage quoted, his solution of the dilemma involves recognition of the social role of all the sciences and extension of, essentially, the scientific method to the problems of human welfare (moral as well as material) and of ethics.

The book is a collection of chapters from previous books and of separate essays, all but one of which have been published elsewhere. In date they range from 1924 to 1945. Although all chapters bear on somewhat the same basic human problems, the arrangement may confuse the development of Otto's thought. The last chapter (followed by a peroration as "conclusion") is actually the earliest writing in the book and is less mature, also shows less insight into science, than several previous chapters on nearly the same subject. A change in approach is visible, for instance, even in the short time between writing "Realistic Idealism" (published as a book chapter in 1940) and "Scientific Humanism" (1943). The book of 1940 was suspicious of science, even antagonistic to it, and proposed an antidote in philosophy—Otto's philosophy, that is, geared to observation of nature and to human realities. But this is, in fact, rather a scientific than a classically philosophical approach, as becomes explicit even in the title of the later essay. There he more clearly espouses not a philosophical remedy for the ills of science but an extension of scientific method "to moral and social problems of every kind."

Since 1943 some of the dangers stressed by Otto have become more acute. Physical science is triumphant and is becoming more and more a destructive or enslaved instrument of the state. (This possibility was inadequately evaluated by Otto, who earlier favored forms of control of science by society which are particularly open to this danger of perversion.) On the other hand, the social respon-

sibility of science has been increasingly recognized by scientists, including many of the more thoughtful physical scientists, and there has been an intensification of effort to interpret scientific results in human, including ethical, terms.

Another recurrent theme in these rather miscellaneous essays is that of the nature of man and especially the problem of the fundamental "goodness" (in some sense) or dignity of man *versus* his basic sinfulness or vileness. Some dominant religions demand the latter view: hence "original sin" and the requirement of redemption. A similar orientation has been derived from scientific emphasis on the animality of man and his origin from lower forms of life. Oddly enough, orthodox theology has rejected this apparent support for its view. It wishes to consider man vile, but not for that reason. Now many scientists believe that their findings refute the scientific denigration of man's basic nature. (Julian Huxley, for instance, attacks the fallacies of the "Nothing-but" school, as in "Man is nothing but an animal," and Maslow insists that the deepest and in a sense most primitive human impulses may also be the most desirable and ethically best.) Thus science, for these students, and religion, for those who believe in original sin, no longer support the same view for different reasons but emphatically contradict each other as to the non-sinful or sinful nature of man. Although he does not express this issue quite so flatly, the same sort of trend results in Otto's biting attack on authoritarianism and supernaturalism in religion. The fact that this essay is the only one in the book not previously published (it was read from manuscript at a conference in 1943) may be significant, even though Otto does not name the particular churches that he clearly had in mind.

The most important point about all this is probably the implication that even such traditionally theological problems are subject to study by scientific methods. They move, indeed, within the proper province of science as science matures beyond preoccupation with the merely physical and loses its strange obsession that objectivity of judgment, which is necessary to scientific method, also means ethical neutrality, which makes science non- or even anti-human. Otto does not put things in quite this way. He is, after all, a philosopher and not a scientist. But he considers much of classical philosophy, its more speculative, not phenomenally controlled parts, as sterile and empty of real subject matter, and when he pleads that scientific method be applied to the still living parts of philosophy he does, in effect, extend the domain of science over this field. This is the next needed step in the struggle of man toward a truly rational interpretation of the universe and of himself.

The scientist may ask one final question. If these subjects are considered a proper concern of science and subject to scientific

method, why is the book written by a philosopher and not a scientist? The answer is that such a study requires a new kind of scientist and that few have yet appeared. Philosopher Otto should be the forerunner of ontologist X of the future, a scientist.

The first issue of such a volume in a thirty-five cent newsstand edition is also a hopeful sign for man's intellectual progress.

G. G. SIMPSON

The History of Nature; by C. F. von Weizsäcker. Pp. vi, 192. Chicago, 1949 (The University of Chicago Press, $3.00). Translated by F. D. Wieck.—Here a young man of great scientific genius unveils beliefs and convictions that have inspired his researches and have kept him sane in the whirlpool which was Nazi Germany. The book is essentially a philosophic treatise dealing with problems of the natural sciences. Its philosophy is not systematic, nor is it presented in a manner aiming at specific goals. But the sincerity of the discourse, the depth of concerns for issues that lie conventionally outside the disciplines of natural science, make the book impressive and memorable.

The subject matter of the book attests to its unusual character. For it contains chapters on "Life," "The Soul," Man's "Outer and Inner History" besides the more customary topics of time, space and astronomy. Interesting facts, many of them new to this reviewer, are included in the purely scientific parts of the book, all of which are pleasant to read and comprehensible to the layman. The author is convinced of what he calls the "historic character of time." His time is similar to Bergson's; its progress is regulated by the Second Law of Thermodynamics, and it has a beginning as well as an end. The reasons for such beliefs are simply presented in this book.

Much space is devoted to the development of stars. This is the subject which will doubtless elicit greatest delight from the reader; it is well presented and beautifully illustrated by plates as perfect as any I have seen.

In final appraisal, one might say that Weizsäcker's book immerses the elements of modern physics and astronomy in the deep current of existentialism which characterizes European philosophy of our time.

HENRY MARGENAU

PUBLICATIONS RECENTLY RECEIVED

Introduction to Parasitology; by Asa C. Chandler. New York, 1949 (John Wiley & Sons, Inc., $6.00).

Practical Spectroscopy; by C. Candler. London, 1949 (Hilger & Watts Ltd., $6.10).

Principles and Practice in Organic Chemistry; by Howard J. Lucas and David Pressman. New York, 1949 (John Wiley & Sons, Inc., $6.00).

Laboratory Manual and Problems in General Chemistry; by A. W. Laubengayer. New York, 1949 (Rinehart and Company, Inc., $2.25).

Friendly China; by Bailey Willis. Stanford, California, 1949 (Stanford University Press, $5.00).

Organic Chemistry; by G. Bryant Bachman. New York, 1949 (McGraw-Hill Book Company, Inc., $4.25).

Oscillations of the Earth's Atmosphere; by M. V. Wilkes. (Cambridge Monographs on Physics). New York, 1949 (Cambridge University Press, $2.50).

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Surface Tension and the Spreading of Liquids; by R. S. Burdon (2nd ed.). (Cambridge Monographs on Physics). New York, 1949 (Cambridge University Press, $2.50).

Seismicity of the Earth and Associated Phenomena; by B. Gutenberg and C. F. Richter. Princeton, N. J. (Princeton University Press, $10.00).

Electronics: Experimental Techniques; by W. C. Elmore and M. L. Sands. National Nuclear Energy Series, Division V, vol. 1. New York, 1949 (McGraw-Hill Book Co., Inc., $3.75).

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Boron Trifluoride and Its Derivatives; by Harold S. Booth and Donald R. Martin. New York, 1949 (John Wiley & Sons, Inc., $5.00).

Multi-enzyme Systems; by Malcolm Dixon. New York, 1949 (Cambridge University Press, $1.75).

Artificial Radioactivity; by P. B. Moon. (Cambridge Monographs on Physics.) New York, 1949 (Cambridge University Press, $2.50).

Ancient Man in North America, 3d ed.; by H. M. Wormington. Denver Museum of Natural History Popular Series No. 4. Denver, 1949 (Paper cover, $1.50; cloth bound, $2.50).

ERRATUM

In the paper by Harold D. Fox entitled "Structure and Origin of Two Windows Exposed on the Nittany Arch at Birmingham, Pennsylvania" (March, 1950), page 157, line 13, should read:

"plunging southwest end of the Nittany arch has been named."

AMERICAN JOURNAL OF SCIENCE

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Manuscripts of articles submitted should follow the recommendations in the pamphlet "Suggestions to Authors" (Lane, 1935) and should be preceded by brief abstracts. They should be checked with special care before being submitted, to avoid changes in proof other than printer's errors.

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